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THE USE OF THE ILLUMINATION SCALE FOR THE DETECTION OF SMALL ERRORS IN REFRACTION AND IN THEIR CORRECTION

BY C. E. FERREE AND GERTRUDE RAND

Bryn Mawr College

There are doubtless many ways in which sensitivity can be added to the acuity test for the detection of small errors in refraction and in their correction. In connection with the problems which we have undertaken during the past eight years involving modifications and refinements in functional testing, three principles have come to light which can be used very effectively to this end. That is, the eye which suffers from an insufficient resolving power shows the following functional defects. (1) An undue lag or slowness of discrimination and of making the adjustments needed for clear seeing. (2) A marked loss in power to sustain the adjustments needed for clear seeing. And (3) an increase in the amount of light required just to discriminate details in the standard acuity object. The devising of test methods based on the first two of these principles has been treated of in former papers. The third alone will be considered here.

The relation of the illumination scale to the detection of small errors in refraction and in their correction may be stated briefly as follows. Insofar as the test-object is concerned, clearness of seeing depends upon the value of the visual angle subtended and the intensity of the illumination. It follows from this that either the illumination scale or the visual angle scale may be used for the detection of errors in refraction, *i.e.*, in the diagnostic procedure either the illumina-

tion may be held constant and the visual angle varied, or the converse. Since the visual angle scale sustains by convention a 1 : 1 relation to acuity while acuity changes slowly with change of illumination for all but very low illuminations, the illumination scale possesses the greater sensitivity for the detection of small errors in refraction—also the greater feasibility of contrivance and manipulation. Used in this way the illumination scale becomes in effect an amplifying scale—somewhat analogous to the use of the tangent scale in detecting small deflections in the magnet system of a galvanometer—and has an advantage in sensitivity in proportion to the amplification. In clinic practice it has been shown to be of particular value in determining the exact amount and placement of the correction of astigmatisms. That is, if the eye has equal resolving power in all meridians, the amount of light required just to discriminate the test-object in all meridians will be the same; if the resolving power is not equal, the amount of light required will be different in the different meridians and different in proportion to the amplification represented by the illumination scale. This gain in sensitivity over the clinic methods is needed in particular to determine the exact amount of the correction in case of high astigmatisms and both the amount and exact placement of the correction in case of low astigmatisms. The checking up of a number of cases shows that the corrections by the clinic methods may be and frequently are off from 0.12–0.25 diopter in the strength of the cylinder and, in case of low astigmatisms, from 5–20 degrees in the placement of the cylinder axis. While errors of this magnitude may or may not be troublesome in the ordinary uses of the eye—sometimes they are very troublesome indeed and, perhaps always tend in time to increase the amount of the defect—they do constitute a much more serious handicap, perhaps an actual disqualification, for work or vocations requiring special ocular proficiencies, *e.g.*, keen acuity, particularly keen acuity at low illuminations; the power to sustain acuity; speed in the use of the eye, especially speed of discrimination and of making the adjustments needed for clear seeing; etc. Moreover, it is safe to say that a considerably

greater amount of light is required as a comfortable and efficient working minimum by the poorly than by the well corrected eye. Indeed our experience with the tricornered relation of intensity of light, resolving power and retinal sensitivity to acuity has impressed us with the relative importance of resolving power in explaining the difference in the amount of light that is required by different people as a working minimum.

The relation of the intensity of illumination to acuity may be illustrated by the curve shown in Fig. 1. This curve represents the average results for four observers, tested by Koenig.[1] In this curve acuity is plotted along the ordinate and intensity of illumination along the abscissa. It will be noted, for example, that a change of from 1 to 9 meter-candles, an increase of 800 per cent. in the intensity of illumination, produced an increase of only 74 per cent. in acuity; and a change of 9 to 100 meter-candles, an increase of 1011 per cent. in illumination, produced an increase of only 28 per cent. in acuity. The amplification within the latter range of illumination is doubtless too great for feasibility of application. That is, too wide a range of illumination would have to be used to compensate for the difference between the resolving power in the poorest and best meridians in the ordinary run of astigmatisms. The range from 1–9 meter-candles is, however, quite feasible and the relation between the two scales gives abundant sensitivity. These values fall within the range given by the apparatus described in our former paper, 0.07–9.5 meter-candles. The testing of a large number of astigmatisms with this apparatus showed that in the majority of cases the minimum amount of light required for the discrimination of the opening in the broken circle (visual angle, 1 min.) in the most favorable meridian was of the order of 1–3 meter-candles; in the least favorable meridian, of the order of 6–9.5 meter-candles.

A very convenient apparatus for using the illumination scale for detecting low astigmatisms and small errors in the

[1] "Ueber die Beziehung zwischen der Sehschärfe und der Beleuchtungsintensität," *Verhandl. der Physikal. Ges. in Berlin*, 1885, 16, S. 79–83.

amount and placement of their corrections was described in a former number of this journal, "An Apparatus for Determining Acuity at Low Illuminations, etc.," 1920, Vol. III, No. 1, pp. 59–71. In this apparatus, it will be remembered, uniformity of illumination of the test surface was secured by

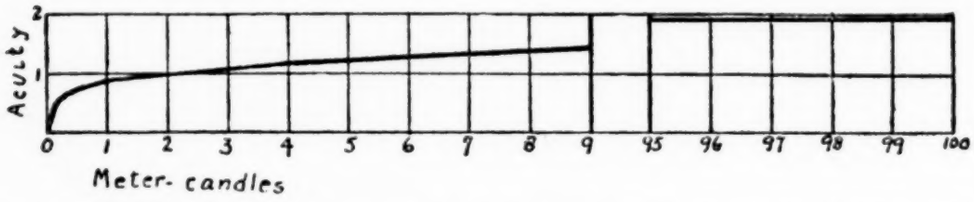

FIG. 1. Showing the relation of intensity of illumination to acuity (Koenig, 4 observers).

projecting upon it the image of an evenly illuminated aperture at the inner end of a projection tube of a lantern or lamp house. In order to secure a uniform illumination of this aperture, the lamp house was lined with opal glass ground on one side, and the aperature itself was covered with a slide of ground glass. The source of light was a round bulb, 100-watt, type C Mazda lamp with its filament well above the aperture to be illuminated, and the changes of illumination were produced by an iris diaphragm placed immediately behind the focusing lens in the projection tube, which reduced the illumination without changing the size or shape of the image. The test-object was a broken circle fastened at the center of a graduated dial the opening of which (visual angle, 1 minute) could be turned into any meridian that was desired. The angle of turning could be read in terms of the divisions on the dial which was graduated to correspond to the readings on the trial frames used in office and clinic work. The results given in this paper were obtained with this type of apparatus. They are fairly representative of the large number that have been obtained.

In the testing and demonstration of the sensitivity and serviceability of the illumination method for determining the exact amount and placement of the correction of an astigmatism the following types of material have been selected: (1) Artificial astigmatisms made with cylinders of low diopter

value. In choosing to include artificial astigmatisms in this work it should be understood that we did not consider the artificial astigmatism the precise functional equivalent of the natural astigmatism. We are too strongly impressed with the possibility that the astigmatic eye may progressively acquire power to compensate in part for its defect to be of this opinion. They were selected because we wished to have in one set of cases an exact knowledge of the amount and location of the defect as a check on the determinations made by the test. (2) Natural astigmatisms without a cycloplegic. (3) Office and clinic cases with a cycloplegic. The difference in result between the most and least favorable meridians or between a true and a false correction have thus far been of a considerably greater order of magnitude with than without a cycloplegic either in case of a natural or an artificial astigmatism. (4) Office and clinic cases, submitted to us by experienced refractionists, in which the apparatus has been used merely to check up corrections already made by the clinic methods, objective and subjective. Among these cases it was comparatively rare to find one in which the minimum amount of light required to discriminate the test-object in the corrected meridian was even approximately equal to that required in the other meridians. Indeed in some cases the difference between the most and least favorable meridian exceeded the range of variation obtainable with the apparatus when provided with the 100-watt lamp. And (5) irregular astigmatisms.

For the artificial astigmatisms three cases have been used.

1. *Low Astigmatisms Produced by Weak Cylinders.*—In this case the minimum amount of light required to discriminate the test-object with the opening of the circle turned into the most and least favorable meridians has been determined; also when turned 5, 10 and 45 degrees from the most favorable meridian. In Table I. it will be noted that in case of an astigmatism produced by a 0.25 diopter cylinder the difference in the light required for the discrimination of the test-object in the worst and best meridians amounted to 107.2 per cent. as an average result for five eyes. At 5 degrees from the

TABLE I

SENSITIVITY OF APPARATUS FOR LOCATING MERIDIAN OF ASTIGMATISM

Astigmatisms produced by 0.25 and 0.75 diopter cylinders.

Observer	Value of Cylinder Producing Astigmatism	Minimum Illumination Required for Discrimination of Test-object (Meter-candles)					Difference in Results Between Best and Other Meridians							
							Meter-candles				Per Cent.			
		Best Meridian	5° from Best Meridian	10° from Best Meridian	45° from Best Meridian	Worst Meridian	5°	10°	45°	Worst Meridian	5°	10°	45°	Worst Meridian
A.....	0.25	0.60	0.88	1.49	1.62	1.62	0.28	0.89	1.02	1.02	46.7	148.3	170.0	170.0
B.....	0.25	1.12	1.75	2.405	2.69	2.69	0.63	1.285	1.57	1.57	56.3	114.7	131.5	131.5
C.....	0.25	0.46	0.60	0.76	0.76	0.76	0.14	0.30	0.30	0.30	30.4	65.2	65.2	65.2
D.....	0.25	1.42	2.115	2.49	2.49	2.49	0.695	1.07	1.07	1.07	48.9	75.4	75.4	75.4
E.....	0.25	1.12	1.88	1.95	2.17	2.17	0.76	0.83	1.05	1.05	67.9	74.1	93.8	93.8
Average.....							0.501	0.875	1.002	1.002	50.0	95.5	107.2	107.2
A.....	0.75	1.30	2.69	3.05	3.05	3.05	1.39	1.75	1.75	1.75	106.9	134.6	134.6	134.6
B.....	0.75	1.81	4.11	4.39	4.39	4.39	2.30	2.58	2.58	2.58	127.8	142.5	142.5	142.5
C.....	0.75	0.60	1.75	1.95	2.32	2.32	1.15	1.35	1.72	1.72	191.7	225.0	286.7	286.7
D.....	0.75	3.05	6.895	6.895	7.60	7.60	3.845	3.845	4.55	4.55	126.1	126.1	149.2	149.2
Average.....							2.171	2.381	2.65	2.65	138.1	157.1	178.3	178.3

best meridian, this difference was 50 per cent.; at 10 degrees, 95.5 per cent.; and at 45 degrees 107.2 per cent. In case of the 0.75 diopter astigmatism, the difference between the worst and best meridians was 178.3 per cent.; at 5 degrees from the best meridian, 138.1 per cent.; at 10 degrees, 157.1 per cent.; and at 45 degrees, 178.3 per cent.

2. *Small Errors in the Placement of the Correction.*—In Table II. it will be noted that in case of an astigmatism pro-

TABLE II

SENSITIVITY OF APPARATUS FOR DETECTING SMALL ERRORS IN THE PLACEMENT OF THE CORRECTION OF AN ASTIGMATISM

Observer	Value of Cylinder Producing Astigmatism	Minimum Illumination Required for Discrimination of Test-object (Meter-candles)			Difference in Result for Correct and Incorrect Placements					
		Exact Placement of Correction	5° Displacement	10° Displacement	Scale Divisions		Meter-candles		Per Cent.	
					5°	10°	5°	10°	5°	10°
A.....	0.25	0.60	1.33	1.62	13.0	17.0	0.73	1.02	121.7	170.0
B.....	0.25	1.12	2.25	2.69	17.5	22.0	1.13	1.57	100.9	140.2
C.....	0.25	0.46	0.60	0.65	3.0	4.0	0.14	0.19	30.4	41.3
D.....	0.25	1.42	1.55	1.88	2.0	7.0	0.13	0.46	9.2	32.4
E.....	0.25	1.12	1.91	2.17	12.5	20.0	0.79	1.05	70.5	93.8
Average..	.....	.....	.....	.....	9.6	14	0.58	0.86	66.5	95.5
A.....	0.75	1.30	2.49	3.05	17.0	22.0	1.19	1.75	91.5	134.6
B.....	0.75	1.81	3.43	4.39	17.0	22.0	1.62	2.58	89.5	142.5
C.....	0.75	0.60	1.42	2.12	14.0	24.0	0.82	1.52	136.7	253.3
D.....	0.75	3.05	6.44	6.44	20.0	20.0	3.39	3.39	111.1	111.1
Average..	.....	.....	.....	.....	17.0	22.0	1.75	2.31	107.2	160.4

duced by a 0.25 diopter cylinder a displacement of the correction 5 degrees from the true position required 66.5 per cent. more light for the discrimination of the test-object as an average result for five eyes; a displacement of 10 degrees, 95.5 per cent. more light. In case of a 0.75 diopter astigmatism a displacement of 5 degrees required 107.2 per cent. more light; and a displacement of 10 degrees, 160.4 per cent. more light. In connection with this table note also the large number of scale divisions between the correct and the incorrect placement of the cylinder. In case of a 5 degree displacement for the 0.25 diopter astigmatism, this difference

averaged 9.6 for the five eyes. That is, since the diaphragm can be readily set to half divisions, 19 settings of the light control could have been made with precision between the values needed for the true correction and the 5 degree displacement. This shows that the sensitivity of the apparatus far exceeds the present possibilities of the precise manipulation of the correcting cylinders.

3. *Small Errors in the Amount of the Correction.*—In the case of a 0.12 diopter error in the correction of an astigmatism, 54.6 per cent. more light was required for the discrimination of the test-object in the worst meridian; in case of a 0.25 diopter error, 108.9 and in case of a 0.75 diopter error, 178.25 per cent. more light was required. These results are shown in Table III.

TABLE III

SENSITIVITY OF APPARATUS FOR DETECTING ERRORS IN THE AMOUNT OF CORRECTION OF AN ASTIGMATISM

Observer	Minimum Illumination Required for Discrimination of Test-object with Different Errors in Amount of Correction (Meter-candles)						Difference in Minimum Illumination to Discriminate Test-object in Most and Least Favorable Meridians					
	0.12 Diopter		0.25 Diopter		0.75 Diopter		0.12 Diopter		0.25 Diopter		0.75 Diopter	
	Best Meridian	Worst Meridian	Best Meridian	Worst Meridian	Best Meridian	Worst Meridian	Meter-candles	Per Cent.	Meter-candles	Per Cent.	Meter-candles	Per Cent.
A	0.60	0.88	0.60	1.62	1.30	3.05	0.28	46.7	1.02	170.0	1.75	134.6
B	1.12	2.17	1.12	2.69	1.81	4.39	1.05	93.8	1.57	140.2	2.58	142.5
C	0.46	0.65	0.46	0.76	0.60	2.32	0.19	41.3	0.30	65.2	1.72	286.7
D	1.42	1.75	1.42	2.49	3.05	7.60	0.33	23.3	1.07	75.4	4.55	149.2
E	1.12	1.88	1.12	2.17	——	——	0.76	67.9	1.05	93.8	——	——
Average							0.52	54.6	1.00	108.9	2.65	178.25

Of the large number of natural astigmatisms tested space will be taken here for the representation of only a few cases.

ASTIGMATISM (WITHOUT A CYCLOPLEGIC)

Case I. (Age 13 Years)

O.D.: Correction by clinic methods, 0.25 cyl., ax. 70°. (Placement of axis could be varied over a range of about 45° and cylinder could be changed to 0.12 diopter without notice-

able change in the results by these methods.) With this correction illumination required with opening of the circle in meridian of cylinder axis, 0.20 m.c.; at 90 degrees from this position, 0.55 m.c.; difference, 0.35 m.c. or 175 per cent.

Correction by illumination method, + 0.12 cyl., ax. 55°. With this correction equal illumination (0.16 m.c.) was required for the discrimination of the test-object in all meridians.

Difference in amount of light required for discrimination of test-object in least favorable meridian for the two corrections, 0.39 m.c. or 244 per cent.

O.S.: Correction by clinic methods, + 0.12 cyl., ax. 180°. (Placement of axis could be varied over a range of about 45 degrees without change in result by these methods.) With this correction illumination required with opening of circle in meridian of cylinder axis, 0.12 m.c.; at 90 degrees from this position, 0.21 m.c.; difference, 0.09 m.c. or 75 per cent.

Correction by illumination method, + 0.12 cyl., ax. 15°. With this correction equal illumination (0.105 m.c.) was required for discrimination of test-object in all meridians.

Difference in amount of light required for discrimination of test-object in least favorable meridian for the two corrections, 0.105 m.c. or 100 per cent.

Case II. (*Age 48 Years*)

O.D.: Illumination required before correction with opening of circle in most favorable meridian, 2.93 m.c.; at 90 degrees from this position, 9.19 m.c.; difference, 6.26 m.c. or 214 per cent.

Correction by illumination method, − 0.50 cyl., ax. 105°. With this correction, equal illumination (2.93 m.c.) was required for the discrimination of the test-object in all meridians.

O.S.: Illumination required before correction with opening of circle in most favorable meridian, 2.35 m.c.; at 90 degrees from this position, 5.25 m.c.; difference, 2.90 m.c. or 123 per cent.

Correction by illumination method, + 0.37 cyl., ax. 137°. With this correction, equal illumination (2.35 m.c.) was re-

quired for the discrimination of the test-object in all meridians.

Irregular Astigmatism

Case I. (*Age 32 Years*)

O.S.: Illumination required with opening of circle turned right, left, and down, 0.97 m.c.; when turned up, 5.25 m.c.; difference for two halves of vertical meridian, 4.28 m.c. or 441 per cent.

Astigmatism (with Cycloplegic)

Case I. (*Age 25 Years*)

O.D.: Correction by clinic methods, + 0.50 S., + 0.37 cyl., ax. 15°. With this correction, illumination required with opening of circle in meridian of cylinder axis, 2.46 m.c.; at 90 degrees from this position, 9.19 m.c.; difference, 6.73 m.c. or 274 per cent.

Correction by illumination method, + 0.50 S., + 0.37 cyl., ax. 30°. With this correction, equal illumination (1.61 m.c.) was required for the discrimination of the test-object in all meridians.

Difference in amount of light required for discrimination of test-object in least favorable meridian for the two corrections, 7.58 m.c. or 471 per cent.

Case II. (*Age 35 Years*)

O.D.: Corrections by clinic methods, − 0.62 cyl., ax. 180°. With this correction, illumination required with opening of circle in meridian of cylinder axis, 2.32 m.c.; at 90 degrees from this position, 9.19 m.c.; difference, 6.87 m.c. or 296 per cent.

Correction by illumination method, − 0.75 cyl., ax. 180°. With this correction, equal illumination (2.09 m.c.) was required for the discrimination of the test-object in all meridians.

Difference in amount of light required for discrimination of test-object in least favorable meridian for the two corrections, 7.10 m.c. or 339 per cent.

Astigmatism (Checking Up of Glasses)

Case I. (*Age 42 Years*)

O.D.: Correction by clinic methods, − .50 S., − .37 cyl. ax. 10°. With this correction, illumination required with opening of circle in meridian of cylinder axis, 2.34 m.c.; at 90 degrees from this position, 7.35 m.c.; difference, 5.01 m.c. or 214 per cent.

Case II. (*Age 45 Years*)

O.D.: Correction by clinic methods, − 0.25 S., − 0.50 cyl., ax. 125°. With this correction, illumination required with opening of circle in meridian of cylinder axis, 2.02 m.c.; at 90 degrees from this position, 6.67 m.c. in one half of meridian, 7.82 m.c. in other half; difference, 4.65 m.c. (230 per cent.) and 5.80 m.c. (287 per cent.). Astigmatism may be slightly irregular.

O.S.: Correction by clinic methods, − 0.50 cyl., ax. 80°. With this correction, illumination required with opening of circle in meridian of cylinder axis, 0.97 m.c.; at 90 degrees from this position, 5.62 m.c. in one half of meridian, 6.23 m.c. in other half; difference, 4.65 m.c. (479 per cent.) and 5.26 m.c. (542 per cent.). Astigmatism may be slightly irregular.

In the above notes on cases we have, for the sake of brevity, used the term clinic methods, instead of specifying in greater particular the tests employed. Where we have made the comparison ourselves between the illumination method and the methods ordinarily employed in office and clinic work, we have used the acuity method, the astigmatic charts, the point of light test and in some cases the ophthalmometer. We have not made frequent use of the retinoscope because of the need of a cycloplegic. The acuity method was used in different ways. In one, patterned after a procedure much employed by the ophthalmologists, some character difficult of discrimination and taxing the resolving power of the eye in as many meridians as possible, such as the letter B, was selected. It was brought to or near to the threshold of discrimination by fogging, by changing the visual angle, by

the use of a graded scale of illumination, etc., in order to make the conditions favorable for a sensitive judgment; and the strength and placement of cylinder was determined which gave the maximum clearness of seeing. In order to decide between doubtful determinations other acuity tasks or tests were imposed. That is, we not only used the acuity test as employed by the practitioner but have endeavored in many ways to add to its sensitivity and precision without sacrificing its distinctive features. However, in collecting the data for the comparison we have preferred to lay the chief stress on the cases in which the clinic testing has been done by practicing ophthalmologists, who have very willingly given us their cooperation, In all cases but one, which have been submitted to us for testing, the physician has himself accompanied the patient, looked after the cycloplegia, and inspected the test procedure at every step, the principle of the apparatus and method having previously been made familiar to him. Due care was taken on both sides that a fair comparison of sensitivities was made.

Doubtless the apparatus can be used in different ways depending upon the experience and preference of the operator. For example, the minimum amount of light required to discriminate the test-object could be determined for one meridian and the setting of the light control be held constant while the test-object is rotated into the different meridians, the observer being required to judge in each case whether the same or more or less light would be required for its discrimination. This would serve as a rough indication of whether or not the eye is astigmatic. The exact meridian of the defect, that is the meridian in which the greatest amount of light is required to discriminate the opening in the circle, could be determined through a series of settings of the test-object and the light control. The placement of the correction having been determined, its amount could be found by the strength of cylinder required to render the minimum illumination needed to discriminate the test-object the same for all meridians, or more roughly speaking for the meridian of the defect and at 90 degrees either way from this position. A quicker and more

feasible method, however, is first to make an approximate determination of the amount and placement of the correction by the clinic methods and employ the illumination method only for a more precise determination. In using this method as a refinement on the clinic methods, the procedure we ordinarily employ is as follows: The patient's eye is fitted with a cylinder of the strength and placement indicated by the clinic tests and the minimum amount of light required to discriminate the opening in the circle is determined in four positions, two in the meridian of the cylinder axis and two in the meridian at right angles to this. If the minima are not equal in these four positions, the cylinder axis is shifted and the determinations are made again, the four positions of the opening of the circle always being in the meridian of the cylinder axis and the meridian at 90 degrees from it. If no placement of the cylinder is found which gives equal minima for the four positions, the strength of the cylinder is changed. The strength and placement of cylinder which requires both equal and the smallest amounts of light for the four positions of the test-object is accepted as the final correction.

The apparatus can also be used to advantage with astigmatic charts of the sunburst type, the radial lines of which are no more than 5 degrees apart, in the preliminary approximate determination of the axis of the defect. In this case the procedure would be to reduce the illumination until only one or perhaps two of the lines stand out clearly. This would give a sensitivity roughly speaking of about 5 degrees, and requires little more time than is usually consumed in the use of the astigmatic charts.

In our own work we have found that the apparatus would be very helpful even if it were used only to check up the corrections made by the clinic methods and were not employed further as an aid in finding out the exact amount and placement of the correction. For example, but a very few minutes are required to determine with it whether any given correction equalizes or levels up the resolving power of the eye in the different meridians. The advantage of a checking method which is definite and at the same time feasible,

can readily be appreciated by any one who has tried to decide by the clinic methods in any wide range of cases just what should be the exact amount and placement of the correction of an astigmatism. The method has its chief value perhaps in those cases in which it is particularly difficult to make a decision by the clinic methods, that is, in determining the exact amount of the correction in cases of high astigmatism and both the amount and placement of correction in case of low astigmatisms. The simple character of the judgment, namely the mere indication of the direction in which the opening of the circle points instead of the more difficult task of deciding under the comparatively rough conditions of the office and clinic test whether this or that placement or strength of cylinder gives the clearer vision, together with the objective check on the correctness of each judgment, also contribute to make the method especially valuable in case of children, and the subjective, unintelligent and untrained type of adult. A further advantage of the method as worked out in connection with the present apparatus is its great sensitivity for the detection of irregular astigmatisms. The lack of satisfactory tests for this troublesome defect is generally conceded.

THE BACKWARD ELIMINATION OF ERRORS IN MENTAL MAZE LEARNING[1]

BY JOSEPH PETERSON

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In another paper the writer has shown that an animal through sheer probability will, with a continued series of efforts, reach the food box in a maze; and further, that on the basis of frequency of exercise (if we grant the operation in learning of certain other factors appearing to be indispensable to learning) the errors of entrance to blind alleys should be gradually eliminated in what has been called the backward order, that is, the errors near the food box should first disappear, and the others should be eliminated in order, speaking generally, from this point back toward the entrance place in the maze. In the earlier paper errors were interpreted as entrances to blind alleys; return runs not bringing about such entrances were not counted in the error score. It was shown that the first blind alley is passed, on the basis of probability solely, $\frac{3}{4}$ of the times that it is encountered in forward runs by an animal, and that the animal is turned back by it toward the entrance $\frac{1}{4}$ of the times. The next blind is likewise passed $\frac{3}{4}$ of the times that it is encountered, or $\frac{3}{4}$ of $\frac{3}{4}$ of the number of times that the first blind is reached. Speaking generally, in n trials the mth blind from the entrance would be passed in forward runs $(\frac{3}{4})^m$ of the number of times that the first blind is reached. The tenth blind, for example, would be passed $(\frac{3}{4})^{10}$—or a little more than 1/18—of the number of times that the first is encountered.

This matter is, of course, vastly complicated by part-return runs. Disregarding these part-returns for the sake of simplicity, and assuming that each return run will take the

[1] Read in part in Section H of the A. A. A. S., St. Louis, Dec. 30, 1919.

animal back to the starting place in the maze, *i.e.*, to the entrance, we find that when any blind alley is passed n times the one next before it will, on the basis of pure chance, be passed $4n/3$ times. "In general, in a maze with y blind alleys the number of times that the first would be passed in forward runs in n trials is $(4/3)^{y-1}$ *times* the number that the last would be passed, *i.e.*, $(4/3)^{y-1}n$." [1] The last blind would be passed n times, as each trial ends only when this blind alley is passed. In a maze of ten blind alleys, therefore, the last blind would be passed n times in n trials, the next before it $1.33n$ times, and the eighth, seventh, sixth, fifth, fourth, third, second, and first, would be passed $1.78n$, $2.37n$, $3.16n$, $4.22n$, $5.62n$, $7.50n$, $10.00n$, and $13.33n$, respectively.

On this basis it was found, on the above assumption of complete returns, that the ratio of entrances into the respective blind alleys *to* passing them successfully, whether entered or not, would be as follows:

$13.33n - .25 : 13.33n$, for the first blind alley,
$10.00n - .25 : 10.00n$, for the second,
$7.50n - .25 : 7.50n$, for the third,
$5.62n - .25 : 5.62n$, for the fourth,
$4.22n - .25 : 4.22n$, for the fifth,
$3.16n - .25 : 3.16n$, for the sixth,
$2.37n - .25 : 2.37n$, for the seventh,
$1.78n - .25 : 1.78n$, for the eighth,
$1.33n - .25 : 1.33n$, for the ninth, and
$n - .25 : n$, for the tenth.

It will be noticed that this ratio of entrances to passes constantly decreases toward the last *cul de sac*, thus favoring 'exercise in running past' *to* 'exercise in running into' the blind alleys, the advantage being increasingly greater with nearness of the blind alley to the food box. It is important to note also that this advantage of exercise in running past blinds over exercise in running into them also increases as n approaches a small number, as 1, 2, or 3, its value during the early trials when greatest progress is made.

[1] Peterson, Jos., 'Frequency and Recency Factors in Maze Learning by White Rats,' *Jour. Animal Behav.*, 1917, 7, 344.

This quantitative explanation is sufficient to account for the backward elimination of errors in the maze, probably, even if we assume only a general motivating effect of the food, operating equally at all parts of the maze, an assumption, however, not intended here. There is apparently no need of assuming any so-called retroactive association, the existence of which in any case of learning may well be regarded as not established.

In the other paper a question was raised regarding the results of an experiment by Hubbert and Lashley,[1] showing that errors made by entrances into blind alleys numbered with even numbers in Fig. 1, errors of their type I., were eliminated in less than two thirds as many trials as were errors of entrances into blinds numbered in odd numbers. It will be noticed that the errors of type I. are made by the animal's running past the place where it should turn, while the other type of error, type II., is caused by a turn in the wrong direction. The serial backward elimination of errors of type II. was found in the main to agree with the results of Miss Vincent, Carr, the present writer, and others; while no such serial elimination of type II. resulted. This apparent contradiction to the general rule of backward elimination was explained tentatively in our earlier paper as due to the fact that when an animal is just correcting in the last stages the errors of running into blind alleys, it frequently is thrown into confusions (probably by changes in the general kinæsthesis) that result in entrances into later blinds which it has already learned to avoid under normal conditions. Since the animal learns at an early stage in the game, comparatively, to keep the forward direction on emergence from blinds[2] and also to orient itself in general toward the center of the circular maze, as was found by Hubbert and Lashley, it would be expected that a combination of these factors or tendencies would throw it into errors of type I. in parts of the maze nearer the food box than are the sources of the dis-

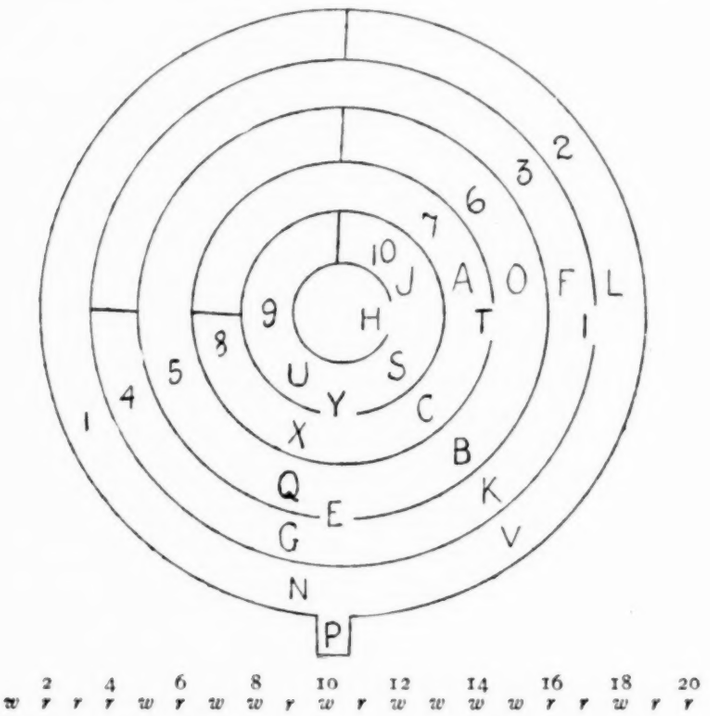
[1] 'Retroactive Association and the Elimination of Errors in the Maze,' *Ibid.*, 1917, 7, 130–138.

[2] Peterson, Jos., 'Behavior Monog.,' Ser. No. *15*, 1917, 24 ff.

turbances. It is our purpose here to present results of an experiment on human subjects with what we have chosen to call a mental maze, a method which eliminates all such confusing spatial factors as those just mentioned.

The Experiment

The present form of procedure was arrived at after considerable experimentation. First, choices of right or left were presented to the subject at the successive stages through



2 4 6 8 10 12 14 16 18 20
w r r r w r w w r w r w w w w r r w r r

Fig. 1. The Mental Maze and Chance Order Schedule.

the maze; it was then found that the different parts of the maze should have a constant designation, so each part was lettered in a meaningless order as shown in Fig. 1. It was desirable to present each pair of letters at any bifurcation simultaneously, but this required an elaboration of apparatus that seemed unadvisable for the practical and wide use of the experiment, though it is still on the program for further experimentation. Tentative experimentation showed that a simpler form of procedure giving reliable results is possible, provided that a pure chance order of presentation of the two

alternatives be followed, and that the same emphasis and tone of voice be used throughout. It was therefore decided to follow a chance order schedule or program, exactly the same for all subjects. Accordingly a program was prepared and written just below the drawing of the maze, as shown in the figure. The letter *r* means to call out first the right letter of the two alternatives, and *w* means to call out first the wrong letter. The numbers occurring just above these letters were found to save much time in determining at the beginning of each new trial just where to start, as will be made clear later.

The experimenter, *E*, sits behind a screen with his finger on the schedule of *r*'s and *w*'s. *S* is given the following instructions to read:

> Do you know what a maze is? It is a winding way to some goal, but it has many blind alleys which will lead to error if you enter them. I have drawn a maze which I will show you when I am through with all the subjects that I shall use in this experiment. Wherever there is a choice of two alleys, each alley is designated by a letter. No two parts are lettered the same, and the letters have a constant position in the maze. Beginning at the entrance of the maze, I call out two letters at a time, and you are to choose one of them; then I call out two more; and so on. Whether the right or the wrong letter is called out first is a matter that is determined wholly by flipping a coin, that is, by chance; so you needn't attend to the order. The position of any letter in the maze, moreover, has no relation to its position in the alphabet; letters occur in random order. The problem is to see with how few errors you can get to the goal, and how soon you can learn to follow the true path with no errors at all. You are through when you get to the goal three times in succession without error. You will be told each time when the goal is reached and the number of errors you made in reaching it, but must find out for yourself where the errors are. Remember, accent and order of calling out letters have no significance. Don't attend to them.

After recording *S*'s name and the time of beginning the experiment, *E*, following the program, calls out N–V. If the subject chooses N, the next letters called out are V–P; but if the subject chooses V the first time, I–L are called out; and so on. The letters that the experimenter calls out at any time depend wholly on where *S* has taken him in the maze, but the order of the letters, that is whether the right or the wrong alternative is presented first, is determined by the schedule, which is gone through from beginning to end and repeated in that order till the goal is reached. On *S*'s second trial V is called before N; that is, the experimenter starts with the *second* letter in the schedule. Each successive

trial begins with the letter of its number in the chance schedule, the twenty-first trial beginning again at the first letter, and therefore following the same order as the first trial.

The writer conducted all the experiments personally, and he practiced giving the letters in a monotonous tone, always accenting a little the last letter of the pair, whether or not one of the pair had been included in the preceding pair of alternatives, as is the case whenever an error has been made by the subject. When the subject gets to the goal he is told: "That is the goal;" not, "H is the goal;" for that would unduly emphasize H in his mind. The subject is then told that another trial begins, so that he will know exactly what he is expected to do. He knows nothing of the chance program, or the place in it of the start on any new trial, other than he could infer from the instructions.

In the practical operation of the experiment other precautions were developed. It was found that in spite of the explicit instructions regarding a chance-order procedure, some subjects tried to choose the first letter called out, on failure otherwise, or the second, or the first one three times and then the second one three times, etc. Some even "tried to spell words," so it became necessary to throw out the records of all such students, four in number, as they made no progress at all toward a solution without error. To make sure that *S* was not either consciously or unconsciously following any order, the writer decided to put a dot under every choice of the first letter called out. With practice this was found to be easily done, and to leave a record for study that is accurate in all essential details. To guard against errors in following the schedule, *E* moved his finger forward to the next letter each time while recording *S*'s response; that is, it was found best to reduce the whole process of calling out letters and recording responses to a fixed routine.

The experiment from the first proved very interesting, giving *E* an opportunity to study *in actual operation* and in ever varying detail, association processes that we have been prone too much to consider only in tables of averages, and in curves of results. Several subjects require at least two hours

to complete the test, while a few good ones do it in a surprisingly short time. The best record to date is twelve minutes for the learning, with a total of only seventeen errors. The experiment seems to have important diagnostic values which we cannot consider in the present paper. The student, for instance, who made the remarkable record just quoted is a young Chinese woman of excellent mental ability and unusual initiative and objectivity of mind, as this attitude has been described elsewhere.[1] Students of poor initiative seem to fall into profitless routine that takes them nowhere.

It will add to clearness to give here the record of one subject in a single trial, that is, till the goal is reached. We shall take the first trial of a young woman who completed the experiment in forty-five trials.

Trial 1. *N** *V* L* V* (P) N* V L* V* (*N*) P* *N** V *L** I F* I* (V) (*P*) N* *P** *V I K G* E Q* E** (*K*) (*F*) *K* E *Q** B T A* *C* Y *S H* (goal). 18 errors.

In the above illustration a letter in italics indicates that the first of the alternatives called out to *S* was chosen; an asterisk after a letter indicates an error (*i.e.*, either an entrance into a blind alley on a forward run, or a return toward the entrance on emergence from a blind); and a letter in parenthesis stands for an uncritical choice, made while going toward the entrance of the maze and at a juncture where no error is possible, as we have defined errors. For each of these meanings the writer has a simple mark that he makes while the experiment is in progress. A complete objective record of all the subject's significant responses is thus easily made possible.

Subjects

The subjects whose records we present in this paper were five men and fourteen women. The women and three of the men were students in the writer's section of general psychology. One of the men is an instructor in psychology, and one is a graduate student. Five of the students reacted to a different lettering of the maze from that shown in Fig. I,

[1] Ruger, H. A., 'The Psychology of Efficiency,' *Archiv. of Psychol.*, No. 15, 1910. Peterson, Jos., 'Experiments in Rational Learning,' *Psychol. Rev.*, 1918, *25*, 463.

the purpose of the change being to make sure that results were not conditioned in an important manner by any particular lettering or letter associations.

Results

Table I. summarizes the results of the nineteen subjects in such a form as to show where and in what trials the 2,956 errors occurred. The totals of the columns, at the base of the table, are arranged to show separately (1) the errors of returning toward the entrance of the maze on emergence from blind alleys, (2) the errors of entrance into blind alleys of odd numbers, Hubbert and Lashley's type II. errors, made by turns in the wrong direction, and (3) the errors of entrance into blinds of even numbers, type I., made by wrongly passing a door.

It will be noticed that our experiment, which eliminates the various disturbing and irrelevant spatial factors peculiar to different mazes, gives results that show practically an equality of errors of type I. and type II., as these errors have already been defined. Moreover, the median of the trials by our several nineteen subjects in which the last error at each part of the maze occurred, shows the same fact of equality in difficulty of these two types of errors, as would, of course, be expected on the basis of probability. This median in each case is found as follows: The last entrance to each blind alley by every subject is tabulated and the trial on which it was made is recorded. We thus have nineteen last entrances for each *cul de sac*. The median of each set of these nineteen numbers is found, and the average of the medians of errors of type I. is obtained and compared with the average corresponding to it for type II. errors. The average of these medians is 6.6 in each case. The results from which these averages were derived will be found in Table II. The median in this case is chosen because it is not affected by certain accidental conditions of extreme cases as is the average.

Studies of the individual records, a matter that we cannot consider here, show that not infrequently when errors of

TABLE I

SHOWING THE DISTRIBUTION OF THE 2,956 ERRORS OF NINETEEN SUBJECTS IN THE MENTAL MAZE LEARNING

Trials	Parts of the Maze*																				Total Errors	Av. Errors per S
	P	1 N	V	2 L	I	3 F	K	4 G	E	5 Q	B	6 O	T	7 A	C	8 X	Y	9 U	S	10 J	H	
1	59	67	63	74	20	21	45	52	18	15	13	21	11	18	6	10	5	8	5	9		540	30.5
2	20	23	49	56	12	15	24	38	13	18	10	20	11	18	11	17	4	10		3		372	19.6
3	21	26	14	13	12	21	16	22	6	15	6	17	13	13	6	10	13	17	1	5		267	14.1
4	26	29	17	26	13	22	14	19	6	14	7	14	3	6	7	11	2	8		3		247	13.0
5	23	28	15	21	11	18	10	18	15	18	7	17	10	15	6	12	3	9	1	3		260	13.7
6	21	24	17	19	12	13	12	19	6	6	4	10	3	7	2	6	4	8		4		197	10.4
7	7	14	3	14	7	12	1	6	2	8	17	23		7	8	13	3	7		3		155	8.2
8	6	11	3	7	4	9	4	10	6	11	2	7	2	7		4	2	5	1	2		103	5.4
9	9	10	5	11	3	6	1	8	2	4	3	11	3	5	2	4	3	7	1	4		102	5.4
10	9	12	6	9	6	9	3	8	5	4	3	10	2	8		5		2		1		101	5.3
11	7	12	3	11	2	6	2	9	4	4	2	12	2	10	5	9	2	8	1	1		112	5.9
12	2	9	4	11	1	2	1	5	2	4	1	8	1	9		5		5		1		71	3.7
13	1	5	1	7				5		4		6		7		3	1	4				44	2.3
14	2	6		1	1	3		3		3		5		6		4		3				37	2.0
15	3	6	1	3	1	3	2	6	3	6	2	5	1	5		4		2				53	2.9
16	2	5	1	2	6	6	1	3		2		4		4		4		2				42	2.2
17	3	4	2	4	2	3	2	6	1	5	1	6		3	2	3	2	5				54	2.8
18	1	4		1	2	3	1	4		2		4		3	1	5		2				33	1.7
19	1	3	1	1		1		3		2		2		2		2		2				20	1.1
20	1	3		1		2	1	3		1		1		1		2		1				17	.9
21	2	5		1		2		2	1	3		1				1		1				19	1.0
22		3		3		2	1	2	1	1				1				1				15	.8
23	1	2		1				1				1		1				1				8	.4
24		1		2		1		1		1		1				1		1				9	.5
	From here only one subject continues through eighteen more trials with errors distributed in parts of the maze as follows																						
25 to 42	2	19	2	4	1	3		3	2	3	3	9	2	5		4		16				78	
Ret.	229		207		116		141		93		81		64		56		44		10			= 1,041	
Odd Blinds		331				183				153				161				135				= 963 (type 2)	
Even Blinds				303				256				215				139				39		= 952 (type 1)	
																	Grand total					= 2,956	

[1] In this table the order of lettering is that shown in Fig. 1, used for Group 1. Group 2 responded to a different lettering of parts of the maze, but the parts were identical with those here indicated.

entrance to blinds near the goal are fully overcome under normal conditions, the final stages of correction of errors nearer the entrance (errors which persist longer) occasionally throw the subject into enough confusion to cause additional entrances to blinds later encountered. This agrees with the writer's results on twenty-four white rats, published in the monograph already cited.

Table I. shows also a general tendency for the errors near the goal to be eliminated first. The persistence of entrances to some of these blinds by a few of the subjects, who did not quickly get hold of the principle that repetition of letters already called out means that the path can be shortened, tends to obscure this general backward elimination tendency somewhat. A judgment as to the nature of this tendency from the absolute number of errors shown in this table is, however, to be made with some degree of caution; for the number of errors near the goal is smaller than that of errors near the entrance, in conformity with the expectations based on the law of chance. If learning advances at an equal rate, absolutely, in all parts of the maze, which it may or may not do, we should expect the earlier disappearance of errors near the goal. We shall consider in a subsequent paragraph the question of the relative rate of learning per unit of practice in the different parts of the maze.

Table II. gives the medians of the last trials in which the several nineteen subjects entered the different parts of the maze, and shows, therefore, in a manner different from that employed in Table I., that in general the errors nearer the goal are eliminated first. A graphical representation of these data is given in Fig. 2. Table II. and Fig. 2 also show that errors of return toward the entrance on emergence from blind alleys are eliminated earlier in the learning of the maze than are errors of going into the blinds. The unnumbered parts of the maze—the parts lettered P, V, I, K, E, B, T, C, Y, and S, contain all the errors of return. On the average the return errors were eliminated 2.1 trials earlier than were the errors of blind-alley entrance. The ratio of 2.1 to the median of the total number of trials to learn the maze, 11, is an

appreciable amount, showing that the early elimination of return runs is a general law supported by our mental maze experiment as well as by our experiments on rats in different forms of mazes.

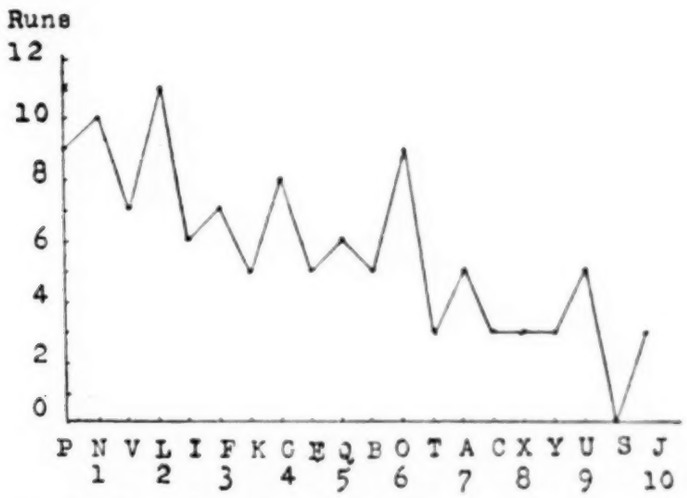


FIG. 2. The letters and numbers under the horizontal line indicate different parts of the maze, and the vertical units indicate the number of the trial on which the last error was made, each number being the median of nineteen subjects. Errors near the goal were eliminated first, and errors of return were eliminated before errors of entrance to blinds.

TABLE II

THE NUMBER UNDER EACH PART OF THE MAZE INDICATES THE MEDIAN OF THE LAST ENTRANCE TO THAT PART BY THE NINETEEN SUBJECTS. NUMBERS ABOVE THE LETTERS STAND FOR BLIND ALLEYS

Parts of Maze																			
	1		2		3		4		5		6		7		8		9		10	
P	N	V	L	I	F	K	G	E	Q	B	O	T	A	C	X	Y	U	S	J	H
9	10	7	11	6	7	5	8	5	6	5	9	3	5	3	3	3	5	0	3	

There are yet other facts pointing to the law of early elimination of return runs. On the basis of probability, strictly, there should be just half as many return errors as errors of entrance to blinds, since the chance is 1 : 1 that a return run (using the term "run" in the mental maze as we have used it in respect to the animal maze) will be made on the subject's emergence from a *cul de sac;* and if a special habit of keeping the forward direction does not develop, we should expect this ratio of 1 : 2 of the number of return errors *to* the number of blind-alley-entrance errors, to remain constant. Table III., which gives these two kinds of errors

separately as well as combined, shows that this ratio, column (4), rapidly decreases from .83, the highest value it has, to 0. This fact, that the forward direction habit develops much

FIG. 3. Units on the base line are trial numbers, and those on the vertical line indicate the ratio of the number of return errors to that of blind-alley-entrance errors. Ratio starts above the expected .50 and rapidly decreases, showing the development of the habit of keeping the forward direction in the maze.

faster in maze learning than do the avoidance-of-blind-alley habits, seems to be of much importance in a proper understanding and analysis of maze learning, though until of late it has been wholly overlooked. This change in the ratios represented in Table III. is shown graphically in Fig. 3.

Table III. and Fig. 3 show clearly, it should also be remarked, another tendency of importance to maze learning, and probably to any kind of actual learning as distinct from mere development of skill in a mechanical performance. These data show that our human subjects had a strong tendency, persisting through the first six trials, to stay by the old and familiar rather than to venture into the novel, a tendency that in this case directly hindered learning and, as the individual records show, differentiated markedly between the good and the poor maze learners. This tendency is shown in the table and graph by the fact that the ratios mentioned

TABLE III

COMPARISON OF THE TWO IMPORTANT KINDS OF ERRORS THROUGH SUCCESSIVE TRIALS IN THE MENTAL MAZE (19 SUBJECTS)

Trial	(1) B. A. Errors	(2) Return Errors	(3) Total Errors	(4) Ratio (2) to (1)	Trial	(1) B. A. Errors	(2) Return Errors	(3) Total Errors	(4) Ratio (2) to (1)
1.....	295	245	540	.83	13.....	41	3	44	.07
2.....	218	154	372	.71	14.....	34	3	37	.09
3.....	159	108	267	.68	15.....	40	13	53	.33
4.....	152	95	247	.63	16.....	32	10	42	.31
5.....	159	101	260	.64	17.....	39	15	54	.38
6.....	116	81	197	.70	18.....	28	5	33	.18
7.....	107	48	155	.45	19.....	18	2	20	.11
8.....	73	30	103	.41	20.....	15	2	17	.13
9.....	70	32	102	.46	21.....	16	3	19	.19
10.....	68	33	101	.49	22.....	13	2	15	.15
11.....	82	30	112	.37	23.....	7	1	8	.14
12.....	59	12	71	.20	24.....	9	0	9	.00

exceed, in the first six trials, the .50 to be expected on the law of probability. The expected ratio is very greatly exceeded in the first trial, in which the effects of the learning were least marked, reaching .83 or 166 per cent. of what is expected by chance. The individual differences with respect to this tendency are so noticeable that the writer feels the tendency will be much more marked in the case of groups of subjects of low mental caliber than in that of subjects of fair ability. Whether this difference among individuals could be made helpful in diagnostic work is a question that we must leave for the present time, one that seems to be worth following out in separate experiments by students having considerable contact with lower grades of mentality. The tendency thus to repeat unprofitable responses in the maze seems not to be very different from the perseverative tendency noted by various experimenters on low grade and on certain types of disorganized minds. Smaller differences in this regard between normal subjects should not, however, be exaggerated as to their significance. What we need just at present is more data.

It seems, then, that the first important step in learning the maze is that of keeping the forward direction, of venturing into ever new choice alternatives. Some of the more clever subjects seemed early to recognize explicitly that return was

unprofitable, and therefore to guard against it; but often the avoidance of the return seemed to come with profitless repetition more or less unconsciously. Even when the principle of infallible assistance to keeping the forward direction was not explicitly recognized, as was ascertained by quizzing the subjects after the experiment was over, subjects often *behaved as if they had the principle in mind.* The principle is that of choosing in any subsequent choice the one of the letters that occurred as one of the alternatives—the rejected one—in the previous presentation. For example, if the experimenter calls out N and V and the subject chooses N, then V must be one of the next alternative choices; if then V is chosen, L and I are presented. If I is chosen L is not repeated, but if L is chosen I must be repeated with V for the next choice. The rationale of this is not, of course, evident even to the best subjects, for they do not see the maze; but they often notice the simple *fact* of such repetition of one of the letters after certain choices which they soon learn to be erroneous, and the absence of it with other, or correct, choices, and consciously and rationally govern themselves accordingly. But, and this is the interesting thing after all, subjects who do not have such explicit recognition of the principle, nevertheless show in their behavior that they are governed by it, as they must be to take this first step in mental maze learning. They, at least, show no recognition of the principle in their unsolicited remarks during the experiment, or in answers to questions seeking evidence of such recognition, after the learning process has been completed. As Swift and other experimenters have found regarding the unconscious adoption of better methods and movements in ball-tossing, so we find here, that our subjects were often forced by the general fruitlessness of their reactions to adopt other modes of response resembling principles or rules of action, without any explicit recognition of the principles. The explicit recognition of a principle often comes after the student has unconsciously followed it for some time. Not infrequently the principle considered here is adhered to only in part, and reactions that fail to follow it throw the subject out of keeping

the forward direction. Thus a subject given the choice between T and O may choose O, and when the next pair of alternatives, T and B, is presented he may by some accident choose B, after which, following the rule closely, he is taken back entirely to the starting place in the maze. This error occurred occasionally whether or not the subject explicitly recognized the principle of choosing the letter repeated. It may even be doubtful, as a study of rational learning by the writer seems to indicate it is, whether explicit recognition of a necessary principle in any case of learning is really a *sine qua non* of the learning. This question we cannot follow out further in the present paper.

Another point of interest may be considered here. From our statement on the second page, of the relative number of times that each blind alley would be passed in each trial by a subject following strictly the law of probability, we can figure how many errors may be expected at each part of the maze by our total of nineteen subjects, and can then compare this expected number with the actually obtained number. On the basis of chance there should be two thirds as many errors of entrance into the several blinds as there are runs made past them; for the chance of entrance to any blind is 1 : 1, and the chance of passing the blind, whether or not it is entered in 3 : 1. But here we are at once confronted by two difficulties. First, as a result of the learning that each subject shows increasingly toward the end of his practice, the number of runs past the blind alleys will, of course, greatly increase proportionately over the number of errors of entrance into them. But if learning goes on in equal proportion to the amount of exercise in all parts of the maze, we may obviously disregard this fact that the errors at each bifurcation gradually approach zero as the learning approaches perfection. If learning goes on more rapidly, proportionately, at one part of the maze than at another, this fact ought to reveal itself by relatively fewer errors all told at this point. Entrance into the blinds would discontinue earlier, comparatively, or would at any rate occur less frequently in proportion to exercise at the place in question, than at other places in the maze. We thus avoid the first possible difficulty.

The second difficulty that we meet in figuring the expected number of errors at the several parts of the maze is a statistical one. It will be recalled that we found the relative number of runs past each blind alley in the forward direction in n trials to be as follows from the first to the tenth blind, respectively: $13.33n$, $10.00n$, $7.50n$, $5.62n$, $4.22n$, $3.16n$, $2.37n$, $1.78n$, $1.33n$, and n. The determination of these numbers was based, however, on the assumption of complete return runs. In our present experiment, as in most experiments on animal learning in the maze as well, return runs are seldom complete. An animal returning from the tenth blind, for example, is very apt to enter a blind on its return run and to get started again in the forward direction as it emerges from this blind. Our coefficients of n are therefore too great for the blinds near the entrance or beginning place in the maze, and it is no simple matter to correct these coefficients from the effect of the assumption of complete return runs; for an animal in the maze may be sent back and forth many times and in diverse ways, according to the expectations of probability, before reaching the food box. The difficulty is not an insuperable one, however.

On the basis of chance one half of the runs *reaching* any blind will become entrances into it. The entrances to the tenth blind will, therefore, equal one half of $1.33n$, the runs past the ninth blind, or $.67n$. Or otherwise derived, the number of entrances to any given blind is equal to two thirds the number of runs past it, the forward direction runs only being considered, since, as has been shown, the runs past equal three fourths the runs to it ($\frac{1}{2} + \frac{1}{4}$), and the number of runs into it equals one half the runs to it ($\frac{1}{2} : \frac{3}{4} = 2 : 3$). Thus $\frac{2}{3}$ of n, is $.67\ n$, the number of entrances to the tenth blind. It is to be remembered that on the basis of the criteria of errors adopted in the present study—any entrance to a blind on a forward run or a return toward the entrance of the maze on emergence from any blind entered on a run in either direction, being an error—there can be no errors of entrance to blinds on return runs. On the basis of these facts we have worked out Table IV.

TABLE IV

THIS TABLE SHOWS THE RETURN RUNS PAST EACH BLIND ALLEY FROM THOSE NEARER THE GOAL-END OF THE MAZE, ON THE ASSUMPTION OF COMPLETE RETURN RUNS FROM EACH BLIND. IT ALSO SHOWS THE NUMBER OF ERRORS EXPECTED AT EACH BLIND ON THE LAW OF CHANCE, CORRECTED FROM THE EFFECTS OF THIS ASSUMPTION. SEE TEXT FOR EXPLANATION

	The Blind Alleys of the Maze									
	1	2	3	4	5	6	7	8	9	10
(1)	13.33*n*	10.00*n*	7.50*n*	5.62*n*	4.22*n*	3.16*n*	2.37*n*	1.78*n*	1.33*n*	*n*
(2)	8.89*n*	6.67*n*	5.00*n*	3.75*n*	2.81*n*	2.11*n*	1.58*n*	1.19*n*	.89*n*	.67*n*
(3)	4.44*n*	3.33*n*	2.50*n*	1.86*n*	1.41*n*	1.05*n*	.79*n*	.59*n*	.44*n*	.33*n*
(4)	.025*n*	.03*n*	.04*n*	.06*n*	.08*n*	.11*n*	.14*n*	.19*n*	.25*n*	Returns from 10th
(5)	.04*n*	.06*n*	.08*n*	.11*n*	.14*n*	.19*n*	.25*n*	.33*n*		" " 9th
(6)	.08*n*	.11*n*	.14*n*	.19*n*	.25*n*	.33*n*	.44*n*			" " 8th
(7)	.14*n*	.19*n*	.25*n*	.33*n*	.44*n*	.59*n*				" " 7th
(8)	.25*n*	.33*n*	.44*n*	.59*n*	.79*n*					" " 6th
(9)	.44*n*	.59*n*	.79*n*	1.05*n*						" " 5th
(10)	.79*n*	1.05*n*	1.41*n*							" " 4th
(11)	1.41*n*	1.86*n*								" " 3d
(12)	2.50*n*									" " 2d
(13)	5.68*n*	4.22*n*	3.15*n*	2.33*n*	1.70*n*	1.22*n*	.83*n*	.52*n*	.25*n*	Totals
(14, 3)	4.44*n*	3.33*n*	2.50*n*	1.86*n*	1.41*n*	1.05*n*	.79*n*	.59*n*	.44*n*	.33*n*
(15)	10.12*n*	7.55*n*	5.65*n*	4.19*n*	3.11*n*	2.27*n*	1.62*n*	1.11*n*	.69*n*	.33*n*
(16)	1.00*n*	1.00*n*	1.00*n*	1.00*n*	1.00*n*	1.00*n*	1.00*n*	1.00*n*	1.00*n*	1.00*n*
(17)	11.12*n*	8.55*n*	6.66*n*	5.19*n*	4.11*n*	3.27*n*	2.62*n*	2.11*n*	1.69*n*	1.33*n*
(18)	8.34*n*	6.42*n*	5.00*n*	3.90*n*	3.08*n*	2.45*n*	1.97*n*	1.58*n*	1.27*n*	1.00*n*
(19)	5.56*n*	4.28*n*	3.33*n*	2.60*n*	2.05*n*	1.63*n*	1.31*n*	1.05*n*	.85*n*	.67*n* = 23.33*n*

The first row of the table shows the relative number of runs past each blind in the forward direction, on the assumption of complete returns. The next row gives the expected entrances to the several blinds in these forward runs. The third row shows the number of return runs expected from each blind on emergence of the subject from the blind. It is clear that one half of the choices on emergence from the blinds will be in the wrong direction. We must figure, then, the relative number of returns from the tenth blind that will *pass* the ninth, the eighth, the seventh, the sixth, the fifth, the fourth, the third, the second, and the first blind, respectively. Obviously, of the $.33n$ returns from the tenth blind, three fourths of this number, or $.25n$, will get past the ninth blind; three fourths of these, or $(\frac{3}{4})^2$ of $.33n$, will get past the eighth blind, that is, $.19n$ runs; $(\frac{3}{4})^3$ of $.33n$, or $.14n$ will pass the seventh; and so on, as shown in the table. Only $.025n$ of the $.33n$ returns from the tenth blind will pass the first blind. The same procedure is followed for the return runs from the ninth, the eighth, the seventh, the sixth, the fifth, the fourth, the third, and the second blind. The results of these calculations are clearly indicated in the table. Then summarizing all these returns past each blind in their respective columns, we get row (13), which when added to row (14, 3), the returns from the several blinds, gives us for each blind the total returns past or from it toward the entrance, corrected from the effects of the assumption of complete returns, row (15). Now since each trial must end with one more forward than backward run past each blind, it is clear that to these totals we must add n, to get the *corrected number of forward runs past each blind according to chance only*. This gives us row (17) in the table. But in this result we get $1.33n$ runs past the tenth blind alley on our assumption of n trials, which is impossible. We must therefore reduce the values of row (17) by the ratio $n : 1.33n$, or $3 : 4$, by which process we get row (18). Thus having the realtive number of runs in the forward direction past each blind in the maze, we can easily find the expected number of errors of entrance into each blind according to our present criteria and conditions of experimentation.

Since the ratio of errors at each blind to runs past the blind is $\frac{2}{3}$, as we have already seen, we need only take $\frac{2}{3}$ of the values given in row (18). In row (19), then, we have the expected number of errors of entrance into each blind from the first to the tenth, corrected from the errors of any assumptions not in agreement with the conditions of the present experiment.

We can now proceed to compare our obtained distribution of errors of entrance into blind alleys with the distribution expected on the basis of chance. The sum of all the expected errors as given in the eighteenth row of Table IV. is $23.33n$. Dividing 23.33 into 1915, the total number of blind-alley-entrance errors made by our nineteen subjects, we get 82.08, the value of n for the present calculation, as is evident from the equation,

$$23.33n \text{ (expected errors)} = 1915 \text{ (obtained errors)}.$$

We can then determine how our obtained errors should be distributed by chance purely, by multiplying each of the coefficients of n in row (19) of Table IV. by 82.08. Table V. gives the results thus obtained, the expected errors at each blind, and it also gives the number of errors actually found at each blind by our total of nineteen subjects. The ratios

TABLE V

DISTRIBUTION OF ERRORS EXPECTED ON THE BASIS OF CHANCE, AND OF ERRORS ACTUALLY MADE BY NINETEEN SUBJECTS IN THE TEN BLIND ALLEYS OF THE MAZE

Blind Alleys	Errors in the Respective Blind Alleys										Total
	1	2	3	4	5	6	7	8	9	10	
Errors expected	456	351	273	213	168	134	108	86	70	55	1,914
Errors obtained	331	303	183	256	153	215	161	139	135	39	1,915
Ratio, exp. to obt.	1.38	1.13	1.49	.83	1.10	.62	.67	.62	.52	1.41	

of the expected to the actually obtained errors are also given. The larger the ratio is at any part of the maze, the greater has been the learning rate, proportionately, at that place. It will be seen by an examination of the table that the expected errors exceed the actually obtained errors noticeably at the

first three, and the fifth, blinds, and also at the last blind. This we interpret to mean that the subjects had to develop rather specifically the principle of avoidance of blinds—and that roughly the first and the last members of the series of associations to be made were the easiest formed. This is in agreement with results usually obtained in the formation of relatively short series of associations. On the whole, then, transfer effects from one part of the maze to another seem to be slight, despite the fact that a simple rule when once learned in any part of the maze will almost infallibly, if not entirely so, guide the subject through other parts of the maze without return runs. It is true that the rule will not prevent entrances into blinds, but it will prevent return runs, which often increase greatly such entrances. The experimenter believes that the amount of transfer in this sense, and measured as here suggested, will correlate fairly highly with the intelligence of the subject. This, however, is a matter that we can not discuss in the present paper.

It is important to note that this fact of different relative rates of learning at different parts of the maze, has no relation to the backward elimination tendency in maze learning, also found to hold with our subjects in the mental maze, for we are now considering learning in relation to the proportion of actual practice in different parts of the maze; whereas the backward elimination tendency was found to depend on the greater learning coefficients (presently to be defined) of the blinds near the goal-end of the maze. The very low ratio of .52 at the ninth blind is probably due to the many errors of one subject at this point, errors persisting down to the second from the last trial for this subject, that is, far beyond the point of complete learning by all the other subjetcs. Our data, especially those for later trials reached only by a few subjects, need verification on many more subjects, a matter that is not so difficult now that the statistical calculations have been made. The writer, however, believes that the tendency here shown, for the learning to be more rapid at the two ends of the maze, irrespective of the backward elimination tendency, will hold.

With the data shown in Table IV., giving the relative number of errors actually to be expected at the different blind alleys on the law of probability, we are able finally to take still another step of importance to the theory of maze learning: we can improve upon the results shown on our second page, and can find mathematically the relative advantage at each blind of exercise in going *past* over exercise in going *into* the blind. The results of such determinations are given in general terms in the left half of Table VI. The next two columns of the table give the ratios when $n = 1$

TABLE VI

LEARNING COEFFICIENTS AT THE SEVERAL BLIND ALLEYS OF THE MAZE, ALSO COMPARISONS OF ADVANTAGE FOR LEARNING AT THE TENTH OVER THAT AT THE FIRST BLIND ALLEY

Blind Alleys	Learning Coefficients			Value of n	Advantage for Learning at the 10th Over that of 1st Blind Alley for Different Values of n
	In General Terms	When			
		$n = 1$	$n = 2$		
1st.....	$8.34n : 8.34n - .25$	1.031	1.015	1	1.29[1]
2d.....	$6.42n : 6.42n - .25$	1.041	1.019	2	1.13
3d.....	$5.00n : 5.00n - .25$	1.053	1.026	3	1.08
4th.....	$3.90n : 3.90n - .25$	1.068	1.033	4	1.06
5th.....	$3.08n : 3.08n - .25$	1.088	1.042	5	1.05
6th.....	$2.45n : 2.45n - .25$	1.114	1.054	—	—
7th.....	$1.97n : 1.97n - .25$	1.145	1.068	10	1.02
8th.....	$1.58n : 1.58n - .25$	1.188	1.086	—	—
9th.....	$1.27n : 1.27n - .25$	1.245	1.109	20	1.01
10th.....	$1.00n : 1.00n - .25$	1.333	1.143	∞	1.00

and 2, respectively. These ratios we have called the *learning coefficients* at the several blind alleys. The learning coefficient at any blind is the ratio of the expected number of runs past that blind in the forward direction *to* the expected number of runs into it (from either direction). It will be noted that the learning coefficients for the two values of n given in the table gradually increase from a small value at the first blind to an appreciably greater value at the tenth blind. A coefficient of 1.00 indicates no learning possibility, so far as frequency effects—the only ones considered here—are concerned; for

[1] This advantage is found by dividing the learning coefficient at the 10th blind alley by that at the 1st; *e.g.*, $1.333 \div 1.031 = 1.29$.

in the case of a blind with such a coefficient, the exercise in running into the blind exactly equals that of passing it.

It will be seen that the learning coefficients not only constantly decrease from the tenth back to the first blind, but that this advantage for the blinds near the goal constantly decreases with increasing values of n. To make this plain we have calculated the advantages at the tenth over that at the first blind alley, when n is 1, 2, 3, 4, 5, 10, 20, and infinity, respectively. This advantage is found by dividing the learning coefficient of the tenth blind by that of the first, for any value of n. Our results, given in the last column of the table, show a constantly decreasing advantage for the tenth blind over the first as the values of n—that is, the number of trials by any subject—increase. When n becomes very great the learning advantage of the near-goal blinds becomes negligible. This is due to the fact that the learning coefficients at all the blinds approach unity as their limit. This general law of decreasing learning advantage of the tenth over the first blind with increasing values of n is important. It suggests one reason why learning in mazes goes on more rapidly in the early trials (when n is small) than in later ones. Another reason has already been pointed out by the writer in the monograph cited above, namely, that the return runs on emergence from blinds disappear relatively early in the learning of the maze, and with them all the probabilities of error due to re-passing blinds nearer the entrance of the maze.

The table in question also makes plain two minor points of interest in maze learning: first, that the learning coefficients reach practically a zero value, a ratio of unity, earlier in the learning process for the near-entrance blinds than for the near-goal blinds, this being really a corollary of what has already been said; second, that the learning advantage for the near-goal blinds over the near-entrance blinds rapidly increases for low values of n—or in the early trials of any subject—with increase in the number of blinds in the maze. From this latter fact we should expect that backward elimination of errors in the maze would be much more marked in mazes with many blinds than in mazes with but few blinds.

We have yet no d
of difficulty to verif
that mazes do not inc
in the number of blinds.

Summary

We have developed an empirical method of studying analytically one of the kinds of learning used in many laboratories, namely maze-learning. Our method applies only to human subjects, but it enables us to study maze-learning independently of the various spatial factors peculiar to individual mazes, and thereby better to determine the general laws involved. Our results from nineteen subjects show convincingly, not only that backward elimination of blind-alley entrances is the rule in maze learning, but also that return runs on emergence from blinds are eliminated earlier in the process than are blind alley entrances. Subjects learn both consciously and unconsciously to follow a simple rule the value of which develops in their random efforts, a rule that leads them to depart from the unprofitable repetitions and to venture into new parts of the maze. There is no qualitative difference between the reactions of subjects who recognize this rule explicitly and those of subjects in whose minds the rule does not come out consciously. In this tendency to depart from the repetition of profitless choices, to go against the valueless effects of frequency, subjects differ materially, the better learners showing greater initiative than the poorer ones.

The experimental results are at every hand compared with expectations based on the law of probability, and mathematical determinations of the learning coefficients at each of the blind alleys are made. The advantages for learning at the tenth blind over those at the first blind are found, and it is shown that this advantage rapidly decreases with successive trials by each subject. This probably is one reason for more rapid learning in the early trials, another being the fact that return runs are eliminated relatively early as compared with errors of entrance to the several blinds.

A method is worked out also of comparing the distribution

istribution based on
n other experiments
last members of the
here) are mastered more readily
bers between the extremes. Finally it is
that the effects of learning in one part of the maze are
not noticeably transferred to other parts. A number of
problems for further investigation are suggested.

SOME VOLITIONAL PATTERNS REVEALED BY THE WILL-PROFILE

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I

Descriptive psychology has familiarized us with various classifications of temperament and of character-types. The four traditional temperaments have been interpreted and re-interpreted in attempts to fit them into various structural systems. At times, the predisposition to experience a certain *quality* of emotion has been chosen as the basal factor in making the division; at other times, the *strength* and *persistence* of the emotional reaction has been considered fundamental and its quality subsidiary. Again, readiness and vigor of motor response; or, rapidity and smoothness of thought is made the principle of division. That is, in the discussion of temperamental and character types we find a varying emphasis upon emotional, intellectual, and motor factors, with more or less insistence upon somatic conditions. Probably physiological experimentation will in the future furnish us with material for a basal interpretation from this latter standpoint.

The net outcome from reading upon this topic is a feeling that there exists some psychological justification for broad outlines of characteristic patterns; the refinement of these patterns has, however, proceeded largely on a logical basis, which in such a classification as that of Ribot's[1] leads to so great an overlapping of divisions as to raise the question of the existence of any real types. Ribot himself concludes that the majority of men are amorphous (plastic to excess), a character-brand determined wholly by environment; or else partial characters or types, that is, patterned along some one line of professional interest only.

[1] 'Psychology of the Emotions,' p. 380 f.

My purpose in the present paper is to present an experimental approach to the problem. Specifically, I wish to discuss certain volitional patterns which have been revealed through employment of a series of tests designed for a volitional analysis of the make-up of an individual in supplementation of his intelligence rating. The emphasis is distinctly on degree of readiness, vigor, suppleness, and accuracy of motor reaction, and on smoothness, confidence and rapidity of thought reaction. If the tests reveal emotional predispositions they do so indirectly, in connection with a specific combination of volitional traits.

The volitional patterns are given in the form of graphs plotted in terms of the scores on a percentile basis received on the following traits: Speed of movement (measured by speed of normal writing); freedom from load or inertia (measured by the ratio of time of speeded to time of normal writing); flexibility (measured by expertness in disguise of one's habitual hand and in imitation of another's handwriting); speed of decision (measured by time taken to decide which one of a series of paired character traits one possesses); motor impulsion (measured by amount of increase or decrease in size and speed of writing when attention is distracted); assurance (measured by reaction to contraction); resistance (measured by blocked writing); motor inhibition (measured by the length of time one can retard his writing of a phrase); care for detail (measured by accuracy of imitation and by difference in time between a rapid and a slow imitation); coördination of impulses (measured by success in writing a given phrase on a short line in speeded time); perseverance (measured by length of time one employs in preliminary practice and final writing of a disguised hand); revision (measured by amount of time taken in rechecking one's first set of judgments on character traits). Discussion of the individual tests in the series, directions for giving them, and norms for scoring are presented in another connection.[1] Plate I. gives a sample profile.

[1] 'The Will-Profile,' University of Wyoming, Department of Psychology, Bulletin No. 3, 1919.

The vital question in estimating the value of the graph is this: Does it have any general characterologicol significance? Or is its meaning specific, limited to exhibiting an individual's reaction to a particular material? Of the twelve tests, eight, it will be noticed, utilize handwriting in some form. The series includes speeded, retarded, disguised, blocked, and automatic handwriting, slow and rapid imitation of script and speeded writing in a restricted space. In many cases the reaction from this set of tests is somewhat definitely patterned. A relatively high score on the first four tests indicates a quick flexible reaction; on the second four traits, it suggests an aggressive reaction; on the last four, a deliberate, methodical, careful reaction. Study of actual graphs reveals eight possibilities in the way of pattern; three one-peaked patterns, emphasizing any one of the three possible groups of four traits each; three two-peaked patterns emphasizing any two groups of traits; a balanced reaction maintaining about the same level throughout the graph; and, lastly, a zigzag pattern which rises and falls without any definite plan so far as the adopted arrangement of traits is concerned. A pattern may run its course at several different levels. We may now rephrase our question to read: Do these types of reaction reveal anything more than the subject's organization of graphic (writing) habits?

It may seem that the proper answer to this question should be sought in a checking up of the significance of each test by correlation of the results it gives with those obtained by a different sort of test of the same trait. Or the order of merit method might be utilized; a ranking should be obtained for a group of subjects on each character trait, and this ranking correlated with the rank received on basis of a given test. So far as is possible, I hope eventually to check some of the individual tests by these methods. Something has already been done. For many of the character traits there are, however, no tests yet available for correlation. Nor is the method of relative position wholly satisfactory because the persons to whom I must appeal for judgments are unpracticed in passing such judgments, reluctant in attempting

them, and, usually, unequally acquainted with the individuals in the group.

These difficulties in the way of checking the significance of specific tests have induced me to try utilizing the graph itself in a rough determination of the extent to which the tests have general characterological significance or the validity of the pattern as actually a volitional pattern. The results of the investigation now to be recorded are not only positive enough to be highly encouraging but the method has given me material that can be utilized in further revision of the tests, and it has suggested possibilities in the way of working out tests of expertness in character analysis.

At this point I would like to emphasize the statement that both the tests and the scoring of them are still in process of revision. For the normal adult between the ages of eighteen and fifty years, the norms established are fairly satisfactory except for one detail. Where sex and age differences exist to any degree the norms are vitiated by the fact that nearly two thirds of my subjects have been women and in age under thirty-five. It appears from separate tabulation of records that in the case of motor inhibition the central tendency for men is much higher than that for women so that with an equal sex distribution of subjects my norms for this trait would be somewhat different. Furthermore, after a long series of repeated trials on a given group, I have concluded that the third trial of retarded writing is more significant than the first and in the revised scale I shall advise giving three trials of retarded writing, except in cases of excessive retardation. This will necessitate the establishment of new norms. I am also revising instructions so as to rule out, if possible, an automatic reaction to this test.

Other details will be handled in revision. I am checking out, for example, a second method of scoring freedom from inertia. The present scoring does an injustice to very rapid workers who are capable of very intensive spurting.

II

At present, however, I propose to pass over such details and to turn at once to the experiment in question. My

procedure was, at first, as follows: I chose three groups of graphs,[1] twelve obtained from college men, twelve from college women, and twelve from university instructors. A group of twelve was submitted to a chosen reagent with a list of names. His task was to fit each profile with the proper name. The instructions given him are reproduced verbatim.

INSTRUCTIONS

You are to identify, if possible, the Will-Profile that fits each person on the list of names given you.

The profiles are graphs which represent the score attained by a given individual on a number of given traits. The method of scoring is as follows:

A score of 10 is the highest given. It would be received by the upper ten per cent. in a group of 100.

1 would be the score received by the lowest ten per cent. in a group of 100.

The other scores grade between 10 and 1 by equal intervals. The traits scored may be defined as follows:

1. Speed of Movement: Speed of movement relative to size of person, and age.
2. Freedom from Inertia or Load: Tendency to work at one's highest speed without external pressure; little tendency to relax speed; quickness in warming up to a task.
3. Flexibility: Ease and success in readjustment; capacity to modify one's routine reactions.
4. Speed of decision: Quickness in reaching a decision or conclusion. A slow reaction here may be due to caution or conservatism in weighing the elements involved in a situation or be caused by one's being side-tracked by irrelevant matters or by a rambling procedure.
5. Motor Impulsion: This trait refers both to impetuosity and energy of reaction. Consider the ease with which brakes or inhibitions are removed and also the tendency to an explosive reaction when the brakes are actually off.
6. Assurance: This refers to the degree of confidence with which one maintains his opinion against contradiction. A 9 or 10 reaction signifies an aggressive reaction —the burden of proof is thrown on the person who does the contradicting; 7 and 8 are confident reactions but reasons are cited for one's confidence and the burden of proof is accepted; 5 and 6 are tactful reactions; below 5 there is a grading down to complete failure to assert one's own opinion.
7. Resistance: The vigor with which one reacts immediately to a blocking of one's purpose. It grades from a strenuous reaction, to complete passivity in the face of opposition.
8. Motor Inhibition: Capacity to keep in mind a set purpose and achieve it *slowly*. It involves power of motor control, imperturbability, and patience.
9. Care for Detail: Attention to details. This trait is not equivalent to accuracy which usually carries an implication of power of keen analysis. One may posses great capacity for detail and yet lack penetration in the selection of details. Care for detail is more evident in execution of a plan than in cleverness in making a plan.

[1] These graphs were plotted for only ten traits. At the time satisfactory norms for Perseverance and Revision were not available.

10. Coördination of Impulses: Capacity to execute a double task without a preliminary trial; capacity to handle a complex situation successfully without forgetting either factor involved. This trait is probably allied to keeping one's head in a confusing situation.

In studying the profiles, give some attention to the general pattern as well as to the scoring on specific traits. In the case of a quick-reacting person the graph runs high at the beginning of the profile; it will run low at the beginning in the case of the leisurely deliberate type of person who carries considerable load. The central part of the profile runs high for the aggressive person. There is an emphasis of the latter part of the curve for the careful deliberate type.

One may find one-peaked patterns, two-peaked patterns, and balanced patterns *at any level of scoring*. Remember 5 and 6 represent a median score. A zigzag pattern demands particular study as it may represent a modification of the natural reaction by training.

Caution: Do not attempt to identify the profile of some one whom you know only by reputation or of whom you have only a general impression. Some specific acquaintance with an individual is necessary.

If you are in doubt as to the meaning of any point in the instructions, question the experimenter about it.

Actual experiment in connection with the attempted identification of profiles showed that the task required considerable effort of attention and power of analysis. Several of the traits measured, freedom from inertia, for example, were new concepts to the judges. In order to give a satisfactory judgment, the reagent felt the need of much more than a casual acquaintance with the persons whose profiles were being exhibited. Not many observers were intimately enough acquainted with each individual in a group of twelve to make satisfactory inter-comparisons. I obtained, however, one series of twelve judgments from each of twelve reagents,—four reagents for each of my three groups of profiles. Correct identification of profiles ran from zero to five, or from total failure to identify any profile (one reagent) to forty-one per cent. of successful identifications (two reagents). The per cent. of successes for the total of one hundred forty-four judgments (twelve judgments by each of twelve reagents) was twenty-two per cent., where chance success would be less than one per cent.

Much more valuable than these summarizing figures was the unmistakable evidence given by the experiment of several important facts:

1. The more intimately an observer knew an individual, the greater chance there was of his correctly identifying the latter's profile. Such a result not only evidences the general value of the profile in character study, but suggests that frequently it might be used by acquaintances in extension of their everyday knowledge of a person.

2. Even in case of incorrect identifications, the judgments, except for one or two very inexpert reagents, were not dictated by chance. There is often considerable agreement as to the *type* of profile chosen for a subject though the specific identification be incorrect. Such confusions are due to general similarity of pattern in the make-up of certain individuals.

3. Individuals vary greatly in their interest in, and capacity to estimate character traits. With a selected group of judges, successes on the will-profile would run fairly high. In this preliminary experiment, the best records were made by the Dean of the College of Agriculture, whose administrative duties enforce a practical interest in character analysis —and by a student of psychology, who undoubtedly has native capacity in this line.

In modification of the method of procedure in such a way as to be able to utilize the judgments of observers who might know intimately only a few individuals whose profiles had been plotted, and yet to insure a simple method of estimating chance versus actual successes, I decided to submit the profiles in groups of three, requesting the observer to choose which profile belonged to a given individual. Chance success of thirty-three and one third per cent. could be anticipated. In order to isolate certain factors for study, I arranged my material in two series. In Series *A*, I presented a given profile with two others like it in general pattern. This series is obviously a difficult one to pass judgment on. In Series *B*, I presented the same profiles but with contrasting patterns. Series *B* is relatively an easy one to handle. It was possible to enlarge my separate groups to any extent I desired, but the combinations in which any particular profile was presented were, of course, kept constant for all

observers; furthermore, the profiles of men students were compared only with those of other male students; women students compared with other women students; university instructors with university instructors. This latter condition probably rendered the experiment slightly more difficult than if profiles had been taken indiscriminately from any of the groups.

The instructions used in the preliminary experiment were used again, except that the observer was told that after picking the one profile of the three that seemed most satisfactory, he might criticize it with respect to any score he chose. By this means I obtained some valuable suggestions as to possible defects in the tests. A majority of the observers had themselves taken the test some months before, and, occasionally, tried to think what a given individual would do under specific conditions. They had, however, no way of knowing how I had utilized the tests in getting a majority of the scores, and were, in addition, instructed to pass judgment on the general character of the subject. Some of the best series of judgments were given by persons who were wholly ignorant of the nature of the tests.

TABLE I

PER CENT. SUCCESSFUL IDENTIFICATION OF PROFILES

Profiles	Series *A*				Series *B*			
	Number of Judgments	% R. Whole Group	Highest % R. Any Judge	Lowest % R. Any Judge	Number of Judgments	% R. Whole Group	Highest % R. Any Judge	Lowest % R. Any Judge
Faculty	69	33.3	55.5	0	75	78.7	100	0[2]
Men Students	54	44.4	60.0	0	57	71.9	100	0[2]
Women Students	37	51.3	100.00[1]	0	31	58.6	100	0[2]
Total	160	41.3	100.00[1]	0	163	72.3	100	0[2]

The results of this test are given in Table I. Using the total series of judgments it is evident that identification of a profile occurs slightly more frequently than chance would lead us to expect even when the profile is submitted with

[1] Only one judgment given, next highest 75 per cent., (3 R. judgments out of a total of four).

[2] Only one judgment given.

others like it in general pattern; while when the same profile is compared with contrasting profiles, the probability that the correct profile will be selected is fairly high. Still more convincing of the value of the profile in analysis of one's temperamental pattern are the successes achieved by the best judges. Thus under the definitely difficult conditions of series *A*, a judge has been able to identify correctly in one group 6 of 11 profiles (55 per cent.) or in another group, 3 out of 4 (75 per cent.). In series *B*, there are a number of judges who make 100 per cent. of correct identifications.

Study of the table reveals an interesting possibility which an inspection of the graphs confirms. In the judgments on the profiles of University Instructors, the per cent. of successes is, in Series *A*, the lowest of the three; in Series *B*, highest. Such figures would suggest the possibility of this group of profiles being more highly patterned than those obtained from the younger subjects, and hence more easily confused when submitted for identification with similar patterns but for the same reason more easily identified in connection with contrasting patterns.

There can be no question of the fact that certain individuals present a much more highly patterned reaction to the test than do others, and that these patterned profiles are under certain circumstances more easily identified than zigzag patterns. The profile of the methodical, careful, deliberate type is, apparently, more easily identified than that of the quick flexible type, particularly if the latter combine aggressive traits with his quickness of reaction, in which case there is, I believe, a tendency to overestimate the probable score on all other traits. This type is, on the whole, also overestimated for intelligence when a judgment is based by the observer on general impression.

In Table II., I have tabulated the number of right and wrong judgments on a number of individual profiles, choosing those in which there was a number of right judgments even in Series *A*; and, secondly, those in which there was a number of wrong judgments in Series *B*. One other case is included.

First, a word of comment concerning those Profiles which

are correctly chosen even in the difficult series. VI. and *H* are identified by the fact that they maintain a high level throughout the graph; VII. and 15 identified by the low level at which the pattern runs. *F* and 4 represent a very definite type of deliberate, consistent, stable personality, one of the easiest types to identify.

TABLE II

DISTRIBUTION RIGHT AND WRONG JUDGMENTS CHOSEN PROFILES

Profile	Series *A*		Series *B*	
	Right	Wrong	Right	Wrong
VI.	8	2	9	1
VII.	3	1	4	0
F.	5	1	5	0
H.	4	1	4	1
4.	4	1	3	1
15.	3	0	3	0
A.	4	3	4	3
D.	1	2	2	1
P.	1	3	1	3
1.	1	3	2	3
5.	0	3	0	3
XII.	0	9	8	1

The second group of profiles, which are not successfully identified even when presented with contrasting patterns, affords much material for study. Several possibilities demand comment.

1. The general appearance of an individual is sometimes deceptive. Age or physique masks the true nature except for very discriminating observers. Casual observation in classroom or faculty-meetings does not serve for adequate understanding of such a character. Profiles XII., *D*, and *P* may be used in illustration.

Profile XII.—The general impression received from the original is that of vivacity, alertness, quickness, and decisiveness. Subject is small, youthful, buoyant. This exterior masks very great care and deliberateness in forming a judgment and extreme sensitiveness to detail. Close observation reveals some inertia in her make-up.

In Series *A*, her profile is not once chosen correctly. But

there is a curious difference in the profile selected respectively by faculty and by student judges. Her faculty colleagues chose a profile emphasizing quickness and flexibility of reaction. They look for a pattern in which the outstanding feature is speed of every sort. Students, on the other hand, look for aggressiveness of reaction and great care for detail. The correct profile runs high on speed of movement, flexibility, assurance, coördination of impulses and care for detail. It dips at speed of decision, freedom from inertia, and resistance to opposition. The original pronounces it an excellent representation, with a reservation concerning the score for coördination of impulses.

An interesting sidelight on this case comes in the choice of profile made by XII.'s housemate who knows her intimately. She overestimates the qualities of deliberateness and detail and chooses a pattern characteristic of a definitely slow methodical individual of much inertia. This reversal of emphasis in choice of pattern is due in part to the bias created by the judge's own very rapidly reacting, inertialess temperament.

Profile P.—This case is a similar one. I do not know the subject personally; the effect he gives to a casual observer is that of a quick, lively, energetic individual. His profile suggests a very stable, careful and methodical person. That he really is of the latter type is asserted by one of his intimate friends. I should suspect it from the fact that his scholastic record runs much higher than his ranking in the freshman tests would lead one to anticipate, a situation which leads one to expect a high record on the traits in the will-profile that emphasize industry and carefulness.

Profile D represents the contrasting case. Here a quick-reacting careless individual is chosen as deliberate and careful. There are two traits that do indeed suggest such a pattern; *D* runs low on motor impulsion and there is some evidence of inertia. Physically, he gives the impression of being slow-moving, phlegmatic and deliberate. As a matter of fact, as I have determined by many timed reactions throughout a year, his speed of decision is excessively rapid and his care for detail distressingly inadequate. He is not slow in movement but there are no excess movements.

His most intimate friend identifies his profile with no difficulty and pronounces it an excellent likeness; as I should, after a year and a half careful observation. But besides the judgments tabulated in the table, there were five failures to identify this profile in the preliminary series with, usually, choice of a highly contrasted pattern.

2. Occasionally, one finds a profile that is unsatisfactory to all or many reagents, but not for the reason just cited. Possibly, we may find a group of reagents of an unstable type whose pattern might fluctuate considerably with repetition of the test. The wavering reactions of observers on profiles *A* and 1 suggest such an explanation, as, in fact, do the personalities in question. They, as personalities, are in definite contrast to *F* and 4, mentioned above; they are much more complex individuals but also much less reliable. Repetition of the test should show much more pronounced shifts in scores for the latter group than for the former.

3. Some errors in identification arise from a strong tendency on the part of many judges to overestimate the possession by others of certain traits. For example, I found one reagent who insisted that every individual whose profile she was seeking to identify should score 10 for assurance. Students were definitely given to overestimation of scores for faculty profiles. One profile, that of an individual of really exceptional volitional make-up, is chosen in Series *A* in almost every instance in which it is submitted as one of a group of three.

4. That the bias of one's own temperament may lead one to overestimate the contrasting traits in others is evident from my records. I have already given an example of this in commenting upon Profile XII., and in mention of the reagent who overestimated the assurance of the individuals whose profiles she handled. I should expect such temperamental bias to operate particularly in estimating the ranking on care for detail. One who would score 10 would probably underestimate the carefulness of others.

5. As might have been anticipated, unfamiliarity with the terms used in analysis caused some difficulty in passing

judgment on a profile. Motor Impulsion, for example, means very little to a reagent. There is also failure to discriminate with reference to traits. The average observer, for example, fails to discriminate between excess movement and rapidity of movement. Smooth, well-coördinated movements are more rapid than those involving effort or excess of nervous activity but they attract much less attention than the latter.

6. That the profile itself needs correction in several details apart from those mentioned earlier in the paper, is also evidenced by the present investigation. The scores most frequently criticized in the detailed inspection were those for Assurance, and for Coördination of Impulses as a test of keeping one's head. It is possible that the test on which Assurance is scored really measures one's susceptibility to suggestion rather than one's self-confidence. The number of double-checks given in passing judgment upon one's own traits may serve as a better indication of the latter trait. This possibility I shall check out.

The significance of the test on Coördination of Impulses would seem open to criticism, not only on the basis of the present returns but also from indications given by the correlational diagrams, to be reported in the third part of the paper.

In a few cases the use of writing as the test material evidently accounts for inaccuracy in scoring on speed of movement and freedom from inertia. Reagents exceptionally practiced in writing may for this reason give an accelerated reaction; while others may show a specialized retardation of speed due to such inhibitions as writing with the right hand instead of the left hand, in the case of a left-handed person.

In concluding this part of the report, I wish to say a word or two concerning the varying value of different judges. A judgment is obviously inadequate when contradicted by the observer's own reaction when conditions are slightly shifted, as in Series *A* and Series *B*. One may also appeal to the judgment of the group or to that of the best qualified judge in checking over the conspicuous successes or failures

of individual reagents. A number of the most successful reagents are of the slow deliberate type of reaction which in my experience is more often associated with a visual than with a verbal preoccupation. They had in this test the advantage of dealing with a visual scheme—the graph,—and some of them very definitely reacted to the patterns as patterns. One of my verbal-minded judges complained of the presentation of character traits in a visual form that meant nothing to him.

The judges who run low on motor impulsion appear more accurate in their identifications of character than the explosive type does, although, possibly, the latter set the social reaction more frequently. There is, apparently, an obsession on the part of the former group with character analysis, a beautiful confirmation of the suggestion that individuals with a temperament on the defensive are better judges of character than those who temperamentally take the offensive.

III

The general pattern that results from plotting the graph obtained by means of the volitional tests and the statement of the total score do not exhaust the usefulness of the profile in analysis of temperament. We have already encountered the zigzag or non-patterned profile, which leads to the problem of other possible organizations of traits, or of natural or acquired inconsistencies of reaction. This, in turn, involves the question of intercorrelation between tests employed in the Will-Profile. An intensive treatment of the data would, no doubt, make it possible to determine to how great an extent a given factor, such as speed of movement, operated in the various tests, and it might well suggest lines of possible revision so as to introduce either greater variety in the way of material or greater accuracy in details of the scoring.

As it will be impossible to handle the problem exhaustively in the present paper, I shall limit myself to a brief summary of the conclusions of relationship I have drawn from inspection of diagrams obtained by so plotting the distribution of scores

as to show the relationship between any two pairs of measures. The diagrams not only reveal the presence or absence of any gross correlation but also make possible a detailed study of each particular score. When such scores stand for a qualitative reaction, such study may be particularly enlightening. One hundred records, equally distributed in the ten groups scoring from 1 to 10, were utilized for each test. A graph was then made to show the relationship between this test and each of the other eleven. The correlation or lack of it was studied, it will be seen, from two angles, since a selection of one hundred records was made twice for each test.

TABLE III

CORRELATION OF TESTS

	Speed Movement	Freedom Load	Flexibility	Speed Decision	Motor Impulsion	Assurance	Resistance	Motor Inhibition	Care Detail	Coördination Impulses	Perseverance	Revision
Speed of movement		+[1]	+	+[2]		+			−	+	−	
Freedom from load	+[1]			+					−(1, 2)			
Flexibility	+			+[2]	+			+	+[1]	+		
Speed of decision	+[2]	+	+[2]				+				−	−
Motor impulsion			+			+[2]		?		?		
Assurance	+				+[2]		+	+	+		+	
Resistance				+		+		+				
Motor inhibition			+		?	+	+		+[1]	+		
Care for detail	−(9, 10)	−(9, 10)	+[1]		−(10)	+		+[1]		+	+	+
Coördination impulses	+	+	+		?			+	+			
Perseverance	−			−		+			+			+[1]
Revision				−					+		+[1]	

Table III. summarizes my conclusions. Positive correlation is indicated by a plus sign; negative, by a minus; absence of correlation by a blank space. In a few instances correlation, or lack of it, is evident only for certain groups as indicated by figures in parentheses. The question-mark is used where the relationship is obscure. In the case of

[1] High correlation.

[2] Low correlation.

motor impulsion and motor inhibition, this fact will serve to introduce a somewhat detailed discussion of the interrelalationship.

TABLE IV

Own Judgment	Groups					
	I	II	III	IV	V	Total
Impulsive	12(4)	8(5)	15(11)	8(2)	10(4)	53(26)
Deliberate	5(3)	12(0)	5(0)	11(3)	8(3)	41(9)
Qualified	3	0	0	1	2(0)	6
Cheerful	17	18(12)	15(5)	13(6)	19(11)	82
Gloomy	1	2	1	4	0	8
Variable	1	0	4	3	1	9
In Between	1	0	0	0	0	1

A few points in the table deserve particular notice. Where traits in the same group have been scored on reactions differing on the surface completely from one another, the correlation points to a common mental attitude influencing both tests and so far confirms the value of the profile in character-analysis. The striking instances of this sort are as follows: Speed of Movement and Speed of Decision (correlation low); Motor Inhibition and Assurance (low correlation); Assurance and Resistance (high correlation); Motor Inhibition and Assurance; Motor Inhibition and Resistance; Perseverance and Revision (high correlation).

But there are interesting cases of correlation between traits not grouped together in the profile. Thus Flexibility is found to be positively correlated with Motor Impulsion, Motor Inhibition, Care for Detail and Coördination of Impulses. Care for Detail is correlated not only with Flexibility but also with Assurance and with Motor Inhibition. Other interesting examples involve the correlation of Speed of Movement with Assurance and Coördination of Impulses; and the positive relationship between Speed of Decision and Resistance, and between Assurance and Perseverance.

Negative correlations are found for Speed of Movement and Perseverance; Speed of Decision and both Perseverance and Revision. These statements suggest a few comments.

The proper grouping of Flexibility and of Coördination

of Impulses is problematic. The relationship found to exist between these two traits was quite unexpected although suggested by certain profiles I had plotted. Both appear to involve somewhat the same combination of motor control and speed of reaction. Whether or not such a combination of traits is akin to capacity to keep one's head is probably very questionable—a conclusion reached on other grounds in the second part of the paper. But that the test for Flexibility does actually measure capacity for general adaptability seems fairly conclusive, although an effort should be made to distinguish in the case of the disguised hand between a dramatic and a studied disguise, at which point my method of scoring is inadequate. That individuals who are notably inflexible fail conspicuously in this test is guaranteed by a number of interesting records.

The positive relationship found to exist between Speed of Decision and Resistance was also unexpected. I do not think, however, that the reason for it is obscure. Obviously, one factor in a quick counter to opposition is a speedy recognition of just what the situation is. I have noted in another connection that members of one of my groups showed very great force in resistance as soon as they had adjusted themselves to the situation. At first they were confused, "fussed"; they are slow-reacting, somewhat inhibited, assured individuals, but just the people whom we know to be excessively obstinate when once they are "set." Just how to revise the scoring so as to do them justice in this particular I do not see as yet.

The positive correlation between Assurance and Perseverance is I think accounted for by the fact that, in part, one's confidence is based on care in noting details with respect either to one's reasons for choice or to objective circumstances.

The most interesting negative correlation is that found to exist between Speed of Decision and Revision. It appears that those individuals who take the longest time to make their decisions in the first place, also spend most time in revising those decisions when given a second chance.

Let us now consider in detail the curious complications of

motor impulsion and motor inhibition, since such an analysis may serve to indicate the sort of scrutiny necessary in estimating the significance of different combinations of traits.

The diagrams give no certain indication of either positive or negative correlation of these two traits. But study of individual groups indicates that subjects scoring 9 or 10 for inhibition rarely score over 5 for motor impulsion. The reverse is not true; a low score on inhibition does not suggest a high score on impulsion. There are, actually, four possibilities of combination. A subject may score high on both traits; low on both; or high on one and low on the other. These subsidiary patterns are, I believe, of great significance. A low score on both traits characterizes a rather definitely relaxed and unresistant individual; a high score for impulsion with a low one for inhibition, the explosive or impetuous person; a high score for inhibition and a low one for impulsion, the deliberate, or, even, the obstructed type; a high score on both, the vigorous forceful personality. But in this latter group, I find, again, two divisions, as revealed by the type of writing produced under instructions to retard movement to the greatest possible degree. For some, this writing shows signs of great tension; for others, not.

Signs of tension include decrease in graphic size, increased pressure, falling alignment, pronounced tremor, and increased conventionality. In the groups scoring over 7 for retarded writing there is some tendency to increase the size of the inhibited writing. This is due largely to the fact that increase in size is a natural outcome of any throwing of attention upon the production of individual letters. But in a few cases such increase in amplitude, with a noticeable absence of any signs of tension, is due to the fact that the reagent is having recourse to *automatic* writing. The tendency of some subjects to shift to an automatic control even in production of retarded writing was noticed in my early tests, with a consequent warning against it. Conversation during retarded writing was not permitted and penmen were urged to keep to their normal size. Inspection of the graphic product shows, however, that a small group of subjects

executed the instructions with no signs of tension; quite the contrary; they produced a large, light hand characteristically automatic. This group of subjects proved to be those scoring exceptionally high on motor impulsion. A list of them gave me a number of individuals of a colorful dramatic type of personality. By some shift in control—whose mechanism we cannot now go into—it is evident that these subjects solved the problem of retardation by an automatic holding of a mental or motor set so that they achieved success with no signs of the tension so noticeably present in other reagents. Possibly restraint of movement by employment of narrow parallel lines might serve to rule out such a reaction, but I am not confident that it would do so.

I believe that to a very considerable extent it is possible to discover these four types of reaction in characteristics of the normal hand. This possibility gives experimental confirmation to a conjecture of mine,—made elsewhere[1]—that the concepts of motor impulsion and motor inhibition offer us our best tools for graphological analysis of writing.

Plate II. gives us a sample of handwriting characteristic of each of the four classes.[2] I. The hand of the individual high both in control and impulsion is a highly individualized but rapid hand. II. The hand produced by a penman low on impulsion, high on inhibition, gives evidence of tension in its excess of pressure, in its cramped or even crabbed forms, or in its small size. III. The hand high on impulsion but low in inhibition is rapid, light, fluent. IV. A penman deficient in both impulsion and inhibition writes a characterless relaxed hand.

It is instructive in this connection to tabulate from our records an individual's judgment upon himself as impulsive or deliberate. One has a right to expect a certain amount of

[1] 'Graphology and the Psychology of Handwriting,' p. 123f.

[2] Hands I. and II. were chosen for the scale (models for Imitation) as samples of the writing of individuals known to be explosive and deliberate respectively. At the time of choice neither individual had been given the Will-Profile Test. When, later, the record was taken my expectations were confirmed to the highest degree. Hand I. is, written by a penman who maintains an automatic set for retarded writing. Individuals of hyperkinetic make-up whose retarded writing shows tension write a much more highly controlled hand.

agreement between the outcome of the tests and an intelligent individual's rating of himself. One might anticipate that an individual high on impulsion and low on inhibition would check himself as impulsive; one high on inhibition and low on impulsion as deliberate; one's expectation concerning the other two groups (low on both traits, or high on both) would be less definite, so too in case of a balanced reaction.

Table IV. summarizes the judgments of one hundred individuals taken at random, twenty for each of five groups, namely I. High on both impulsion and inhibition; II. High on inhibition, low on impulsion; III. Low on inhibition, high on impulsion; IV. Low on both inhibition and impulsion; V. Medium on both traits. The numbers in parentheses give those who were willing to double-check their judgment because of their confidence in its correctness. I have also included in Table IV., the judgments passed by these individuals upon their cheerfulness or gloominess of outlook.

The table as a whole confirms to some degree the outcome of the tests. Group III., for example, shows a great preponderance of judgments in favor of impulsiveness, with many individuals double-checking this judgment. The case for Group II. is much less pronounced. Turning to my records, I find that of the twelve rating themselves impulsive in Group I., eight were individuals who produced an unmistakably *automatic retardation* of writing, to which reference has already been made; two others were doubtful cases. It is interesting to note that the qualified judgments appear in Groups I., IV., and V.

The most significant fact in the tabulation of Cheerful-Gloomy judgments, is—apart from the large preponderance of Cheerful judgments,—the variable group in III. and the number of those rating themselves as gloomy or variable in IV. Possibly the figures under IV. point to some sense of inadequacy on the part of a number of the group.

The agreement between the reagent's judgment of himself and the outcome of the test might, of course, be much more striking than it actually is. No doubt a certain amount of discrepancy is due to inaccuracy in the scoring for motor im-

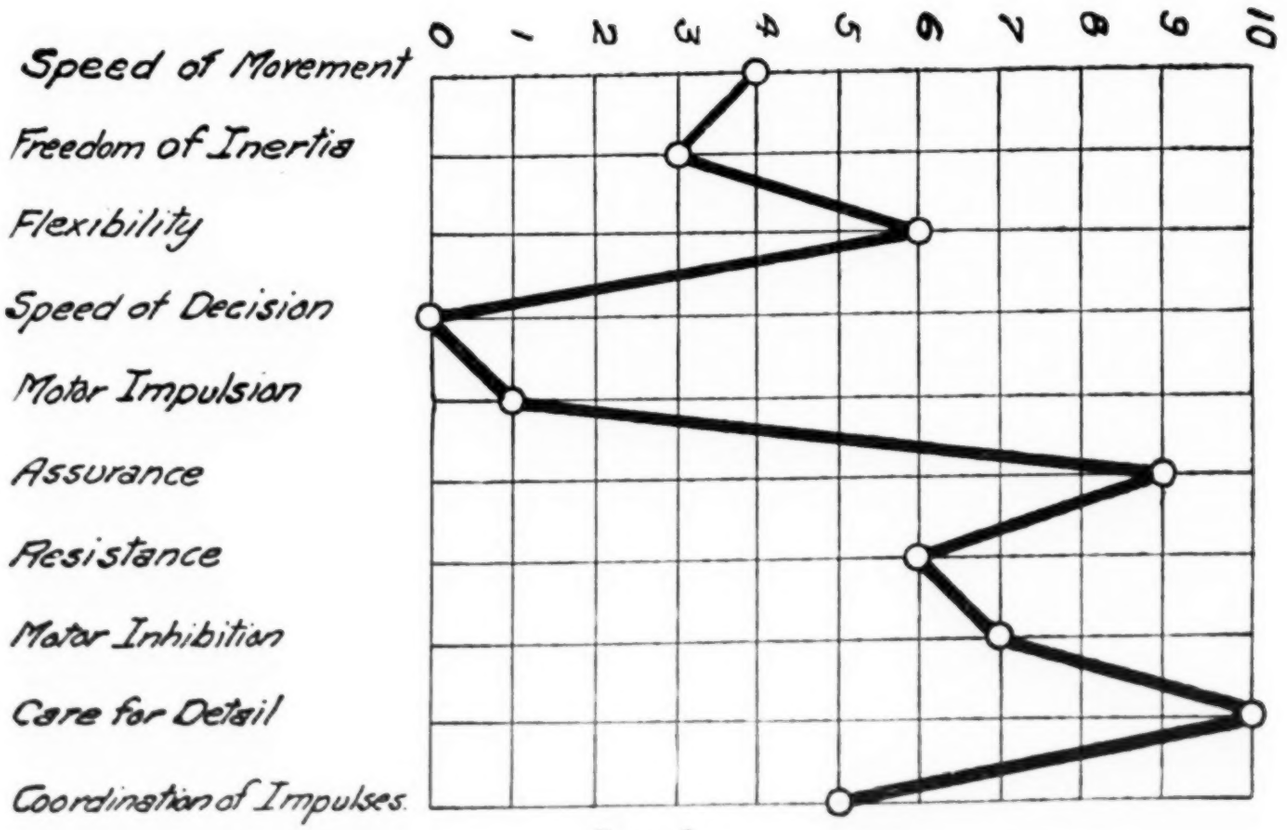

PLATE I.

Very characteristic profile of a scientist, X. Has been picked up from my desk with the exclamation, "That looks like Doctor X!" An intimate associate in the same department makes one comment apropos of his uncertainty concerning significance of score for coördination of impulses. "X," he asserts, "should run high on this trait if handling things; low, if handling people."

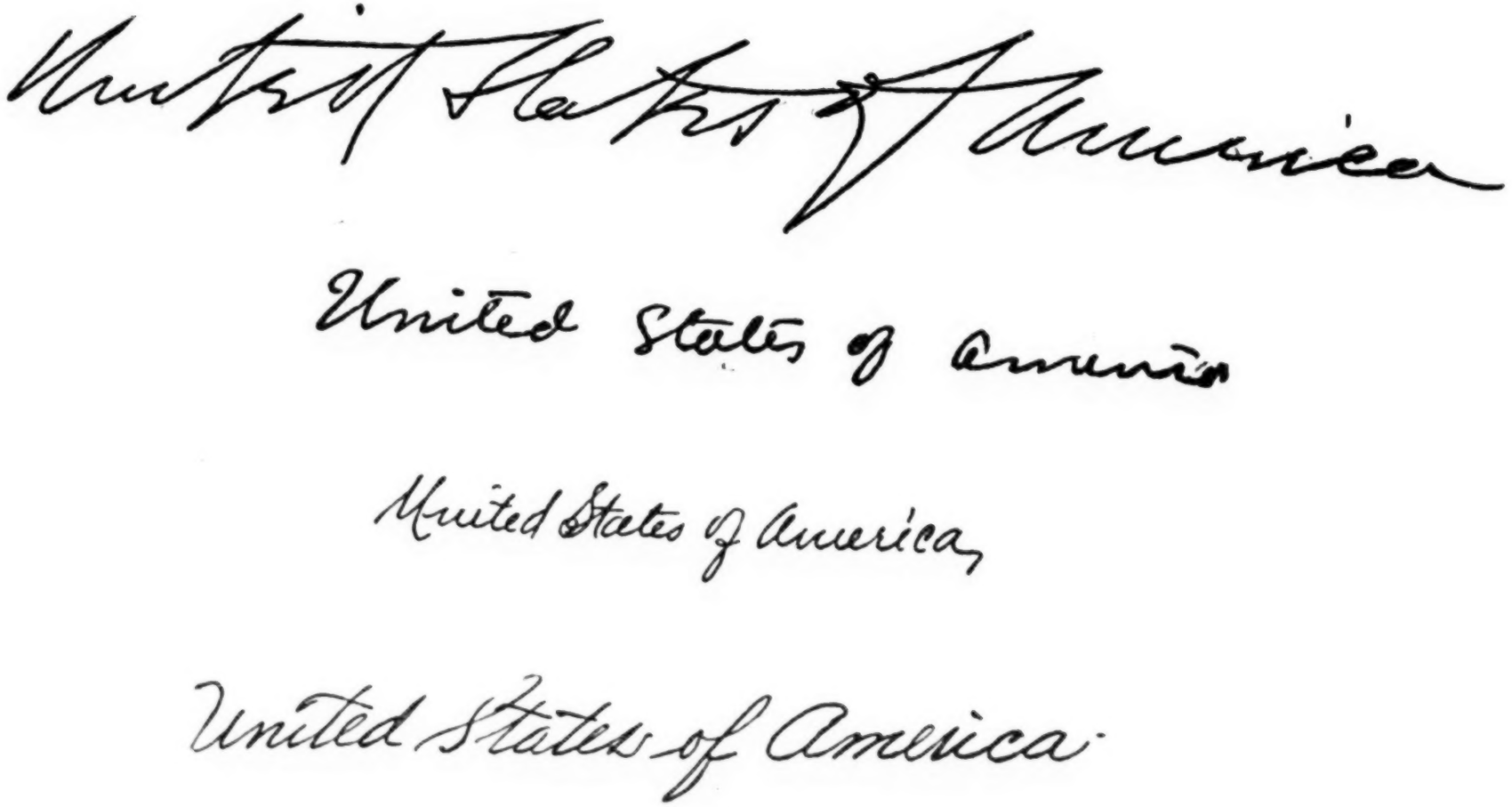

PLATE II

I. Written by an individual scoring high on both Motor Impulsion and MotorInhibition.
II. Written by an individual scoring high on Motor Inhibition, low on Motor Impulsion.
III. The hand of a subject scoring high on Motor Impulsion, low on Motor Inhibition.
IV. The hand of a subject scoring low on both Motor Impulsion and Motor Inhibition.

pulsion, an inaccuracy I hope to remedy by just the sort of analysis here presented, but in large measure the discrepancy is due to individual notions as to the meaning of the terms. One man, certainly far from impulsive in his make-up, who nevertheless so checked himself, explained when questioned that he understood 'impulsive' as equivalent to 'enthusiastic.' He himself never acted without reasons, and in that sense was 'deliberate.' Until character-analysis has developed a technical vocabulary of definitely defined words, we must expect to find ourselves struggling with verbal misunderstandings.

Summary

The outcome of the present investigation indicates that the Will-Profile has considerable general characterological significance and that it can be used to advantage not only in getting the general temperamental pattern of an individual but also in determining the specific combination of traits.

The answer to the question, Does the Will-Profile give you any new information about an individual? is certainly an assured affirmative in the case of a casual acquaintance, and also an affirmative in the case of intimates since it serves the purposes of detailed analysis, just as does an intelligence test. In conjunction with the latter, it certainly affords in many situations, a basis for conservative prophesy.

ON THE 'AFTER-SENSATION' OF PRESSURE[1]

BY R. T. HOLLAND

In 1916 F. L. Dimmick published, from the Cornell Laboratory, an experimental study of cutaneous after-images.[2] The stimulus, a hair of the tension-value of 4.5 gr./mm., was applied to pressure spots localized on the volar surface of the forearm.[3] While the experiments were performed with all due care, it has for various reasons seemed desirable to repeat and extend them. In the first place, Dimmick had but two observers; and, as it happened, the results furnished by these observers differed very considerably. On the whole, the times obtained from the one are eight times as large as those obtained from the other. It is true that the ratios of the times of after-sensation, interval and after-image are approximately constant in both cases, so that the difference is uniform and not casual or variable. Still, it seemed worth while to repeat the experiments with other observers, if only to decide whether or not the extreme types are connected by intermediates. In the second place, there is something paradoxical about a sensory response, to a weak stimulus of 1 sec. duration, which lasts (with the after-image) from 40 to 90 sec. The result contradicts the experiences of everyday life, and suggests the operation of some source of error which the experimental technique failed to control. In the third place, both of Dimmick's observers, though in unequal measure, reported experiences of tickle as accompanying the pressures; and it is not easy to see why that quality should have intervened.[4] We felt, then, that there is sufficient

[1] From the Psychological Laboratory of Cornell University.

[2] *Amer. Jour. Psych.*, XXVII., 1916, 556–569.

[3] The stimulus was so chosen as to affect only the cutaneous and not the subcutaneous organs, a precaution which was apparently neglected by M. H. S. Hayes in her "Study of Cutaneous After-Sensations," *Psych. Rev. Mon. Suppl.*, 60, 1912.

[4] These results are, of course, not peculiar to Dimmick's investigation: see Hayes, *op. cit.* On the other hand, M. von Frey, in his review of the sense of pressure (L. Asher and K. Spiro, 'Ergebnisse der Physiologie,' XIII., 1913, 96 ff.), makes no reference to tickle, even in his discussion of the phenomenon of *Entlastung*.

ground for reopening the problem of the time-relations of the sensation of pressure, and accordingly undertook the experiments now to be described.

It was our intention to work with stimuli of different durations, and also with stimuli of different intensities, some adequate to the cutaneous organs alone, and some involving also the subcutaneous tissues. Circumstances have made it impossible for us to carry out this programme; and the present study is concerned accordingly with the results obtained from a single stimulus applied for a uniform period of time.

Apparatus and Procedure

The stimulus employed was a horse-hair, set in a von Frey hair-æsthesiometer. We wished to use a hair which should affect only the cutaneous organs, should give a definitely supraliminal pressure for all spots and all observers, and should have as nearly as possible the same tension-value as the hair employed by Dimmick. We found such a hair, with the following constants: length from handle, 30.5 mm.; diameter (the hair was sensibly circular in cross-section), 0.2 mm.; force, 0.48 gr.; tension-value, 4.8 gr./mm.[1] Repeated trials with light etherization of the skin showed that this stimulus affected the cutaneous organs only. The stimulus was also positively supraliminal: out of 1,511 applications there were but 27 failures, 19 of which occurred early in the course of the experiment, before the introduction of the reading glass (see below). Since *E*'s uncertainty of manipulation was the reason for recourse to the reading glass, we have little doubt that most at any rate of these 19 blanks are attributable to maladjustment of the stimulus; the remainder of the 27 we can account for only conjecturally, as due to some minimal displacement of *O*'s arm.

The stimulus was applied to the skin by means of the mechanical applicator described by Dimmick. The time of application, 1 sec., was taken by *E* from the swings of a noiseless seconds' pendulum. By way of further control, *E* closed a lip-key at the moment the hair touched the skin, and opened it again as the stimulus was removed. The records of ten series, each one of ten applications, taken at random from the whole number, show the following times:

(1) 0.99 ± 0.04,	(2) 1.09 ± 0.05,	(3) 0.99 ± 0.01,
(4) 1.01 ± 0.06,	(5) 0.96 ± 0.06,	(6) 1.03 ± 0.07,
(7) 1.02 ± 0.07,	(8) 1.06 ± 0.08,	(9) 1.05 ± 0.04,
(10) 1.03 ± 0.05.		

The second was therefore approached with a fairly high degree of accuracy. The procedure is, indeed, less complicated than it might appear; and *E*, after practice, was able to time his applications almost automatically.

The exact localization of the *pressure spots* was secured partly by clipping the surrounding hairs, and partly by entry on a map of transparent celluloid which could

[1] The account of von Frey's stimulus-hairs given by W. H. Howell ('A Text-Book of Physiology,' 1915, 281) is misleading. The pressure-value of the hair is rightly calculated; von Frey, however, calculates, not this pressure-value, but the tension value. See E. B. Titchener, *Exper. Psychol.*, II., ii., 1905, 49.

be fitted to the arm-cast in the manner to be described below. Accuracy of stimulation on *E*'s part was furthered by ink-dots placed just peripherally to the spots and by the use of a large reading glass (magnifying 1.5 diam.). Ten well-marked spots were localized on the dorsal, and ten on the volar surface of the left forearms of our five *O*'s. The rectangular areas within which these spots were included (beginning at a line about 65 mm. from the carpal folds) were of the following approximate dimensions: for D, 30 x 80 mm. (dorsal), 10 x 60 mm. (volar); for H, 25 x 75, 10 x 50; for M, 15 x 50, 10 x 50; for Ta, 20 x 65, 15 x 30; for Tu, 15 x 45, 15 x 45. The smallest distance between spots was 3 mm. (D, M, Tu), the largest was 26 mm. (D); the average distance was 7.5 mm.

The ten spots of the area under experimentation were stimulated once each and once only during an experimental hour. The spots were numbered arbitrarily 1 to 10. The order of stimulation was determined by chance: ten tickets, bearing the serial numbers of the spots, were shaken together, and drawn one by one. This procedure was repeated 10 times, so that *E* obtained a random sequence of 100 stimulations. The sole departure from chance occurred when the numbers of adjacent spots were drawn in immediate succession; in this event the second number was thrown back among the rest, and a new drawing was made. Since there was an average interval of 4 min. between stimulations, there was an interval of at least 8 min. between the stimulations of adjacent pressure-spots.

O's left forearm was held in a carefully molded plaster cast, which included both elbow and hand. The fingers lay in an easy and natural position; and the cast, placed on a low table, allowed the whole arm to rest as comfortably as possible. The edges of the cast were built up to take the celluloid map referred to above; marks were also made upon the plaster, to ensure the precise fitting of the map to the area of stimulation.

O's free (right) hand controlled a key whose pressure and release recorded the appearance and disappearance of sensation or 'after-image.' The two records (*E*'s record of the time of application of stimulus, and *O*'s record of sensation) were made side by side upon ticker tape drawn over a spool by a noiseless motor of practically constant speed and wound upon a kymograph drum; the time-line was given by a Kronecker interrupter marking tenths of a second. The three magnets of the recording apparatus carried eosin writing-points of the type used for recording barometers. *E* threw the motor and interrupter into action by means of a foot-lever. A curtain hanging across the experimental table prevented *O* from seeing arm or instruments.

Five *O*'s served throughout the experiment: Dr. K. M. Dallenbach (D) and Dr. L. B. Hoisington (H), instructors in psychology; Miss M. F. Martin (M), scholar in psychology; and Messrs. S. Takaki (Ta) and S. Tung (Tu), graduate students in the department. Practice was continued for some three weeks before the regular series were begun.

The instructions given to the *O*'s were as follows: "After the usual Ready-Now signals, I shall apply a pressure-stimulus to your forearm. When sensation begins, close the key; when it ends, release the key. Be ready for a recurrence: and if a sensation recurs, close the key again at the moment of its appearance, and release it at the moment of disappearance. Continue until there is no recurrence. Afterwards, give a brief description, in attributive terms, of the pressure or pressures set up by the stimulus." These instructions remained unchanged throughout the experiments, save for a modification introduced in the course of Series II., to which we refer presently.

Experimental

Series I.: Forearm Prone

It is rare that an *O*, in experiments upon the volar surface of the forearm, does not sooner or later complain of the discomfort of his bodily posture. Since we planned a long series of experiments, it occurred to us that it might be better to lay the forearm prone, and to work on its dorsal surface. The suggestion was tried out in a few preliminary series, and was approved by the *O*'s.

As the regular series progressed, it became apparent that the *O*'s fell into two distinct groups. The one, which we shall call the 'regular' group, consisted of D, Ta and Tu. These *O*'s reported times of primary sensation which were of the same order of magnitude as the duration of the stimulus, and noted relatively few and brief recurrences or 'after-images.' The following tables set forth, for these three *O*'s,

Table I

Observer *Tu*

Spot	Sensation	No.	Int.	A.-S.
1	0.56 ± 0.20	(1)	1.5	2.5
2	0.57 ± 0.24	(1)	0.3	0.3
3	0.80 ± 0.40	(2)	0.5 ± 0.1	0.4 ± 0.2
4	0.85 ± 0.39	(1)	1.1	0.2
5	0.81 ± 0.28			
6	0.84 ± 0.50	(2)	2.3 ± 1.0	0.5 ± 0.3
7	0.89 ± 0.36	(2)	1.0 ± 0.3	0.2
8	0.85 ± 0.35	(1)	1.3	0.2
9	0.86 ± 0.33			
10	1.22 ± 0.46			

Table II

Observer *D*

Spot	Sensation	No.	Int.	A.-S.
1	1.3 ± 0.19			
2	1.6 ± 0.43	(1)	1.1	2.5
3	1.8 ± p.38	(1)	1.9	2.5
4	1.6 ± 0.33	(1)	0.5	1.0
5	1.6 ± 0.21			
6	1.8 ± .049			
7	1.2 ± .022			
8	1.4 ± 0.37	(1)	4.6	0.9
9	1.4 ± 0.34			
10	1.7 ± 0.33			

the duration in sec. of the primary sensation (average of 10 applications); then, in parenthesis, the number of times that a given spot showed a recurrence; and then the duration of the blank interval and of the first 'after image.' One table (Ta) is continued to a further stage: it sets forth, again in parenthesis, the number of times that a given spot showed a second recurrence; and then the durations of the second interval and of the second 'after-image.'

TABLE III

OBERSVER *Ta*

Spot	Sensation	No.	Int.	A.-S. 1	No.	Int.	A.-S. 2.
1	1.6 ± 0.15	(7)	2.0 ± 0.32	1.4 ± 0.55	(1)	3.5	6.5
2	1.9 ± 0.40	(4)	1.5 ± 0.72	2.1 ± 0.23			
3	2.1 ± 0.35	(6)	2.1 ± 0.71	1.6 ± 0.40			
4	1.7 ± 0.19	(10)	2.5 ± 0.45	1.9 ± 0.97			
5	2.2 ± 0.44	(7)	2.0 ± 0.58	2.2 ± 0.79	(2)	5.6 ± 3.8	0.9 ± 0.6
6	1.5 ± 0.23	(6)	2.9 ± 0.65	1.6 ± 0.84			
7	2.2 ± 0.64	(4)	2.2 ± 0.90	1.8 ± 0.94			
8	2.0 ± 0.24	(5)	2.4 ± 0.47	1.4 ± 0.57	(1)	3.8	1.0
9	1.9 ± 0.49	(4)	1.7 ± 0.29	2.3 ± 1.35			
10	1.7 ± 0.49	(5)	2.3 ± 0.77	1.4 ± 0.34			

The averages from these results are as follows:

TABLE IV

O	No.	Sensations	No.	Int.	A.-S. 1	No.	Int.	A.-S. 2
Tu.....	(100)	0.82±0.38	(10)	1.18 ± 0.58	0.55 ± 0.45			
D.....	(100)	1.56±0.34	(4)	1.02 ± 1.28	1.72 ± 0.82			
Ta.....	(100)	1.89±0.39	(58)	2.21 ± 0.37	1.74 ± 0.44	(4)	4.62 ± 2.38	2.35 ± 2.08

The results themselves are, it seems, what we might have expected. As regards the primary sensation, the *O*'s differ gradually and within narrow limits; the average times of sensation vary from nearly one second to nearly two seconds. The responses of the spots also differ gradually; the extreme ranges give the ratios 1:2 for Tu, 2:3 for D, and 3:4.4 for Ta. The *O* who shows the longest average time for the primary sensation shows the greatest liability to recurrence; but the recurrence rarely (only in 4 cases) goes beyond a single return, and its average value is slightly less than that of the primary sensation. All in all, the tables wear an appearance of uniformity.

Reports.—D's regular[1] report is of a quality of contact or bright, sharp, clear but weak pressure. This quality is sometimes obscured by, or passes into, a thin wiry prick (10 cases) or a more diffuse tickle or itch (4). Usually the intensity decreases steadily throughout the observation; occasionally it remains steady or decreases until the removal of the stimulus, when it suddenly increases (5). Of the 4 recurrent experiences, 2 are of the original quality (one of these may have been erroneously reported, owing to shift of attention), 2 are diffusedly ticklish or itchy.

Tu, a less practised observer, was somewhat disturbed by visual images. His regular report is of a bright, ticklish contact. This quality is sometimes obscured by, or passes into, 'a sort of prick, a little painful' (9). Of the 10 recurrences (2 of which are reported as doubtful), 7 are of the original quality (one of these is localized aside from the spot), and 3 are of a heavier, stingy nature.

Ta, who is also a relatively unpractised *O*, reports regularly for the primary sensation a weak but clear pressure, which gradually fades out, and only rarely brightens or becomes ticklish when the stimulus is removed. Of the 58 first-recurrences, 46 are described as faint, vague, diffuse, tickish; 12 are at first clear, 'concentrated,' fairly intensive, but quickly pass into the other stage. All 4 of the second-recurrences have the quality of vague, unsteady tickle.

TABLE V

OBSERVER H

Spot	Sensation	No.	Int.	A.-S. 1	No.	Int.	A.-S. 2	No.	Int.	A.-S. 3
1	2.4±0.48	(9)	1.5±0.73	3.5±1.14	(1)	3.5	2.0			
2	3.1±1.10	(8)	1.8±0.95	3.8±1.10	(5)	2.5±1.37	4.7± 1.83			
3	3.1±0.89	(7)	1.5±0.27	4.5±1.96	(4)	1.7±1.27	4.4± 0.76			
4	3.0±0.80	(7)	1.7±0.29	4.0±1.86	(3)	3.1±1.13	2.7±0.55			
5	2.4±0.55	(7)	2.0±1.45	5.6±3.66	(4)	2.4±0.60	6.0±1.71			
6	2.8±0.53	(8)	1.3±0.65	4.1±1.50	(6)	2.1±0.56	4.5±1.34			
7	2.6±0.54	(5)	1.2±0.14	7.4±4.68	(1)	2.7	1.1			
8	2.9±0.82	(7)	2.0±0.82	4.8±1.89	(1)	3.2	2.2			
9	2.1±0.29	(5)	1.5±0.74	3.2±2.04	(1)	2.5	3.5			
10	4.1±0.58	(8)	1.4±0.55	3.8±1.25	(5)	3.8±0.96	4.4±2.00	(1)	0.4	0.8

Our other *O*'s, H and M, belong to what we may call the 'irregular' group.[2] It is, to be sure, possible to make out for these *O*'s tables which show, for the primary sensation, time-values lying on the average under 3 sec. (see Tables V., VI., VII.). But, in the first place, the recurrences are

[1] The reports that we term 'regular' are, of course, summaries of a large number of individual descriptions. All *O*'s, and especially the trained *O*'s, D and H, indicate differences (as between spot and spot) of compactness or sharpness and bluntness or dullness. Since, however, the differences are not pronounced, and since they are normal to groups of pressure spots, we have not thought it worth while to go into further detail.

[2] The group also includes Dr. C. W. Perky, who made 89 observations in this first series of experiments. P's selected average time of primary sensation was (82) 2.38 ± 0.90 sec. The remaining 7 times (nearly 8 per cent.) were long.

TABLE VI

OBSERVER M

Spot	Sensation	No.	Int.	A.-S. 1	No.	Int.	A.-S. 2	No.	Int.	A.-S. 3	No.	Int.	A.-S. 4	No.	Int.	A.-S. 5
1	3.4±1.33	(5)	6.5±2.44	1.1± 0.56	(3)	7.2± 4.83	3.7± 0.37	(2)	3.2±0.60	3.9 ± 0.6	(1)	13.4	2.8			
2	2.8±1.04	(10)	4.2±2.60	1.4± 1.11	(4)	5.3± 1.37	6.6± 9.34	(2)	4.6±0.30	11.7 ±11.5	(1)	8.5	0.7			
3	3.4±1.15	(6)	4.2±1.80	22.9±33.0	(1)	16.6	2.3	(1)	5.5	0.4	(1)	27.3	3.0			
4	3.1±0.94	(6)	6.0±3.55	5.2± 4.42	(3)	3.3± 1.77	2.0± 2.00	(1)	3.7	0.4	(4)	10.1±2.48	3.0±1.58	(1)	0.15	3.7
5	2.2±1.18	(7)	6.1±1.61	5.5± 3.45	(3)	22.8±18.37	16.1±17.30	(3)	9.0±6.83	16.0 ± 7.9						
6	3.1±1.41	(9)	5.2±4.42	21.3±25.70	(7)	6.5± 4.01	2.8± 1.46	(4)	3.8±1.94	3.3 ± 1.0						
7	2.5±0.96	(4)	4.0±1.35	4.1± 3.11	(2)	2.5± 0.67	1.6± 0.15	(2)	3.3±7.20	1.92± 0.3						
8	2.2±0.49	(8)	6.7±1.29	3.9± 4.92	(4)	4.2± 1.70	4.2± 1.97	(3)	2.4±1.40	27.0 ±18.9	(1)	3.2	0.6	(1)	1.50	3.6
9	2.3±1.10	(9)	2.6±2.14	13.5±17.00	(4)	14.7±11.31	3.9± 2.15	(1)	1.0	5.8	(1)	2.2	5.8			
10	2.0±0.79	(8)	7.0±5.47	9.4± 8.45	(4)	6.6± 5.55	12.1± 6.26	(1)	3.8	3.6	(1)	5.3	3.8			

TABLE VII

O	No.	Sensation	No.	Int.	A.-S. 1	No.	Int.	A.-S. 2	No.	Int.	A.-S. 3	No.	Int.	A.-S. 4	No.	Int.	A.-S. 5
H	(100)	2.88±0.78	(71)	1.59±0.66	4.37± 2.01	(31)	2.63±1.04	4.26±1.64	(1)	0.4	0.8						
M	(100)	2.71±1.16	(72)	4.99±3.16	0.21±11.16	(35)	8.30±0.64	5.73±4.80	(20)	2.0±2.8	3.4±7.4	(10)	7.0±4.9	2.0±2.7	(2)	0.8±0.7	3.6

more marked: H gives 71 of the first and 31 of the second order; M gives 72 of the first, 35 of the second, and 20 of the third order. And secondly, the tables do not record (as the earlier tables have done) the results of 100 consecutive experiments; the values are selected. It is characteristic of the members of this group that the ordinary or regular times for the primary sensation are replaced at irregular intervals by times of much greater length, times ranging for H between 10.1 and 55.9, and for M between 10.5 and 204.2 sec. H gives 8 per cent. of these long times, indifferently from the 10 spots, and M gives 27 per cent., principally from 6.[1]

Since H, in particular, is a practised *O*, we thought, on the recurrent appearance of the long times, that something must be at fault with apparatus or procedure. Fatigue could hardly come into play; there were but 10 observations in the hour; and in point of fact the long times, from first to last, were as likely to occur at the beginning as in the middle or at the end of an experimental sitting. H's reports gave us no cue. M, however, sometimes reported with the long times: 'tingling as when the arm is asleep,' 'twitching of muscles under skin like gooseflesh,' 'tickle with pulse,' 'pulse in wrist,' 'pulse in fingers.'[2] Although these remarks were by no means frequent, we nevertheless imagined that they might point to an interference with circulation due to the prone position of the arm. We therefore again worked carefully over the surfaces of the casts, to make them if possible still more comfortable for the *O*'s; and we divided the experiments of an hour into groups of 4, 3, 3 (or 4, 4, 2), between which the arm was taken from the cast and the *O* was free to move. These changes were of no avail; the long times persisted. We therefore determined to continue the observations until series of 100 'regular' times had been completed (Tables V., VI.) and then to repeat the work with the arm supine. It was possible that with this position of the arm the long times would disappear; it was possible also that the reports of the *O*'s would give, by comparison, a clue to their conditioning.

[1] Yet M's spot 8, which responded irregularly to but 2 out of 12 stimulations, jumped in these two instances from its average of 2.2 ± 0.49 sec. to 17.3 and 109.5 sec. respectively!

[2] P also spoke occasionally of 'throbbing' and 'pulse' in the arm.

Reports.—H's regular report is of a weak or moderately intensive neutral pressure, punctiform or of very limited area. The quality sometimes passes into, or is interrupted by, pain (8 cases) or tickle (8). Usually the intensity either decreases steadily throughout the observation or shows a rise followed by a continuous decrease; occasionally it remains steady or decreases until the removal of the stimulus, when it 'flares up' (6). Of the 71 first-recurrences, 41 repeat the original quality, but weaker, and of diffused and indefinite area; 32 (there is a slight overlap) possess a brighter, ticklish quality; and 2 are painful. Almost invariably these experiences fade out gradually. The second-recurrences are still weaker, show a greater tendency to become ticklish, and occasionally are 'very like imagery.' Their disappearance, when recorded, is always gradual.

The reports for the long times are naturally incomplete. There is fluctuation of quality, of intensity, and of area. What is more to the point is that, on several occasions, H refers to these experiences as 'perceptual;' they are 'patterned'; there may be a 'granular core' with 'a fine and fuzzy edge, extended longwise,' and the pattern may shift instably, with change of localization.

M is an observer somewhat more practised than Ta and Tu, but far less practised than D and H. She reports a light, clear contact (68), a tickle (17), a delicate prick (4), or a dual combination of contact with tickle or prick. The quality either fades out gradually, or shows an increase of intensity followed by a steady decline. There is no report of terminal increase of intensity, though in a few cases the disappearance of the primary sensation is said to be sudden or abrupt. The recurrences are almost always described as diffuse tickle, 'spread on the arm,' 'not localized at spot'; the tickle is occasionally blended with contact, and very rarely contact is reported alone.

Primary sensations and recurrences are, one and all, accompanied by visual images, —so M remarks in a summary report; but these images are seldom recorded, and seem not to have distracted attention from the skin. M also reports a strong reflex tendency, when tickle is aroused, to take her right hand from the key and move it to the left arm. The tendency seems to be regarded at least as a secondary criterion of the presence of tickle.

In her reports for the long times, M mentions, as a rule, a temporal fluctuation of qualities, in which two or all three of the familiar members of the group (contact, tickle, prick) appear; sometimes there is a 'third' or a 'fourth' quality which is left unnamed; every now and then warmth or cold flashes in. The attributive report is cut across by references to pulse, arm asleep, tingling, gooseflesh. There are a few cases in which the perceptual nature of the experiences is clearly brought out: M speaks of a wide spread of tickle in which 'points come and go associated with bright spots in [visual] image'; of contact and diffuse tickle accompanied by 'visual image of belt of tickle on arm'; of tickle coming in 'as ring round the original spot touched, as I imagine'; of a blend of tickle and contact 'spreading right and left in visual image, with prick-sensation separate.' Here the visual imagery plays a more active part.

Second and later recurrences, with both regular and long times of primary sensation, are frequently given with a question-mark: 'may have been imaginative,' 'not sure whether real or imaginal.'

Series II.: Forearm Supine

The following Tables VIII.–XI. show the results obtained from the three *O*'s of the previous regular group.

TABLE VIII

OBSERVER Tu

Spot	Sensation	No.	Int.	A.-S.
1	0.90 ± 0.24	(1)	1.7	0.2
2	0.96 ± 0.20			
3	0.93 ± 0.23	(1)	1.6	1.1
4	0.93 ± 0.14			
5	0.94 ± 0.15			
6	0.98 ± 0.22			
7	0.96 ± 0.20			
8	0.99 ± 0.26			
9	0.97 ± 0.22	(1)	0.7	0.2
10	0.91 ± 0.13			

TABLE IX

OBSERVER D

Spot	Sensation	No.	Int.	A.-S.
1	1.33 ± .016			
2	1.50 ± 0.27	(1)	3.1	1.0
3	1.35 ± 0.23	(1)	1.5	1.0
4	1.60 ± 0.27	(1)	1.5	1.1
5	1.50 ± 0.23	(1)	3.7	1.4
6	1.53 ± 0.26			
7	1.50 ± 0.20			
8	1.80 ± 0.50			
9	1.42 ± 0.18	(2)	1.5 ± 0.17	0.7 ± 0.5
10	1.62 ± 0.18			

TABLE X

OBSERVER Ta

Spot	Sensation	No.	Int.	A.-S. 1	No.	Int.	A.-S. 2	No.	Int	A.-S. 3
1	2.57±0.70	(7)	6.2±1.8	10.2±7.5	(2)	7.2±2.5	10.2±9.4	(1)	0.4	0.5
2	2.90±0.69	(9)	4.8±2.2	9.9±4.3	(1)	7.5	19.6			
3	3.00±0.90	(8)	6.1±2.4	8.7±8.6	(2)	6.0±1.4	10.0±9.6	(1)	3.1	0.7
4	3.00±1.20	(8)	3.1±1.4	7.2±4.6	(2)	7.1±2.6	1.8±1.8	(1)	3.0	0.5
5	3.00±0.86	(7)	4.6±1.0	13.5±7.2						
6	3.50±0.88	(9)	6.8±2.8	11.7±5.2	(2)	5.9±0.6	1.9±0.5			
7	2.80±0.55	(10)	5.4±1.9	11.0±9.5	(3)	8.8±8.5	2.9±1.3	(1)	2.4	0.5
8	3.40±1.00	(8)	5.8±3.6	12.9±1.0	(1)	3.0	4.6			
9	2.50±0.35	(7)	6.9±2.9	8.2±4.5	(1)	5.2	6.8			
10	3.00±1.00	(2)	5.6±0.6	5.2±3.2						

TABLE XI

OBSERVER Ta

O	No.	Sensation	No.	Int.	A.-S. 1	No.	Int.	A.-S. 2	No.	Int.	A.-S. 3
Tu	(100)	0.95±0.19	(3)	1.30±0.4	0.5±0.4						
D	(100)	1.51±0.23	(6)	2.10±0.7	0.6±0.36						
Ta	(100)	3.00±0.85	(75)	6.20±2.5	10.6±6.0	(4)	6.8±3.2	6.3±5.8	(4)	2.2±0.9	0.5±0.6

The tables are very similar to Tables I.–IV. The three *O*'s stand in the same order of rank. Tu has decreased his range to 10: 11; D and Ta have hardly changed in this regard (11:15, 5:7). Ta has increased his average time of total sensation from nearly two to three seconds, and his record of recurrences is fuller, with longer times. On the whole we may say that Tu and D have behaved as they behaved in Series I., and that Ta, while he does not fall obviously out of line with his former record, may perhaps show the influence of some condition tending progressively to lengthen his periods of response.

Reports.—D's regular report is of a contact, or bright, clear pressure, more intensive than in Series I. This quality is sometimes obscured by, or passes into, a prick or wiry pain (13 cases) or a more diffuse tickle (8) or itch (15). Usually the intensity decreases steadily throughout the observation; sometimes it decreases and then increases (11); occasionally it decreases or remains fairly steady until the removal of the stimulus, when it suddenly increases (3). Of the 6 recurrences, one (of which *O* reports that it was probably imaginal) is of the original quality; 4 are diffusedly itchy; one has a tingling character.

Tu's regular report is of a light, delicate, ticklish contact. This quality is sometimes obscured by, or passes into, a bright pain (8). Tu now reports comparatively few visual images; we get, in exchange, reports of intensity. The contact usually disappears gradually; sometimes (9) its intensity decreases and then increases. All three of the recurrences are of the original quality, a ticklish contact.

Ta regularly reports a weak but clear (or very moderately intensive and clear) pressure, which either gradually fades out or (in 22 cases) passes, for the most part abruptly, into something bright and ticklish. Of the 75 first-recurrences, 57 are described as faint, vague, diffuse, unsteady, ticklish; 9 begin in this way, quickly become clearer and more intensive, and relapse into the first stage; 7 begin as dull, heavy and diffuse, and pass into the familiar vague tickle; and 2 only begin as 'concentrated.' Once a sensation of cold is remarked. All of the later recurrences are termed, comparatively, fainter, more obscure, more diffuse, fluctuating; all are of a ticklish quality.

We had hoped that, with change of the position of the arm from prone to supine, the two members of our 'irregular' group would give results comparable with those obtained from D, Ta and Tu. This hope was not fulfilled. In the first series taken under the new conditions with H and M, typical 'long' times (56 and 83 sec. respectively) occurred among the normal short times. Fortunately, one of M's reports in an early series gave us the cue to the situation: it appeared that M was making a voluntary effort to hold the

TABLE XII

OBSERVER H

Sensation	No.	Int.	A.-S. 1	No.	Int.	A.-S. 2	No.	Int.	A.-S. 3	No.	Int.	A.-S. 4
4.4±1.3	(9)	1.7±0.6	5.8±1.0	(4)	2.5±0.4	4.8± 0.7						
3.3±0.9	(9)	1.9±0.9	5.3±2.4	(3)	1.9±0.5	14.9±16.5	(1)	1.0	3.5			
3.3±1.3	(10)	1.4±0.4	7.4±3.0	(5)	2.9±0.9	6.1± 2.4	(1)	0.5	13.2			
4.0±1.4	(8)	2.1±0.8	5.4±1.7	(2)	2.7±0.0	4.0± 1.5						
4.5±1.5	(9)	2.2±0.9	7.5±1.7	(5)	4.0±1.6	4.8± 1.3						
3.9±0.9	(9)	1.8±0.7	4.9±1.9	(1)	3.8	3.1						
4.0±1.2	(8)	1.3±0.4	7.3±3.5	(4)	6.9±6.3	6.0± 2.0	(2)	2.7±2.7	14.9±9.6	(1)	6.5	11.4
3.4±1.5	(8)	1.6±0.4	6.2±1.3	(2)	3.6±1.0	4.3± 1.7						
4.1±1.2	(7)	3.1±2.0	12.2±8.2	(2)	1.3	5.1± 1.4						
3.9±1.4	(8)	1.6±5.8	7.3±2.3	(4)	3.1±0.6	5.0± 1.4						

TABLE XIII

OBSERVER M

Sensation	No.	Int.	A.-S. 1	No.	Int.	A.-S. 2	No.	Int.	A.-S. 3	No.	Int.	A.-S. 4
2.0±0.29	(8)	4.3±3.2	7.0± 9.1	(2)	13.3±1.6	0.7±0.5	(1)	16.0	0.9			
1.8±0.34	(8)	3.6±2.1	5.3± 6.4	(6)	3.6±1.3	2.1±2.4	(3)	9.3±7.4	1.8±0.9	(2)	7.3±4.1	0.2±0.03
1.7±0.38	(8)	2.9±1.8	1.4± 1.3	(3)	3.2±0.4	0.4±0.2	(3)	27.3±2.3	6.6±6.1	(1)	5.1	0.2
1.8±0.54	(8)	3.7±3.0	8.9±10.4	(4)	5.1±5.2	3.6±4.5	(2)	12.2±9.4	1.2±1.7	(2)	2.6±1.2	1.5±0.07
2.3±0.77	(9)	5.8±4.5	2.8± 2.5	(4)	3.6±1.1	1.6±0.7	(2)	1.8±3.6	0.7±0.6	(2)	3.8±1.2	0.7±0.05
2.4±1.00	(9)	6.4±4.7	2.5± 2.6	(5)	4.6±3.7	4.2±4.6	(2)	6.4±5.2	2.2±2.0			
2.1±0.65	(7)	3.4±2.0	1.5± 1.9	(4)	2.4±4.3	0.3±0.1	(2)	8.3±6.4	3.5±0.1	(2)	5.2±2.2	0.3± 0.10
1.8±0.36	(8)	4.2±1.2	6.6±10.1	(5)	6.9±2.3	2.6±1.8	(2)	2.8±0.8	1.3±1.2			
1.9±0.23	(9)	4.8±3.0	2.1± 2.2	(6)	3.8±1.1	0.9±1.0	(4)	5.2±1.2	1.7±1.5	(1)	5.4	0.5
2.0±0.40	(9)	4.2±3.7	13.2±16.0	(5)	4.5±1.4	4.4±5.1	(2)	11.1±7.1	7.3±7.1			

TABLE XIV

	No.	Sensation	No.	Int.	A.-S. 1	No.	Int.	A.-S. 2	No.	Int.	A.-S. 3	No.	Int.	A.-S. 4
H...	(100)	3.9±1.3	(85)	1.8±7.6	6.8±3.2	(32)	3.4±1.5	6.0±2.7	(4)	1.7±1.3	16.6±8.9	(1)	6.5	11.4
M...	(100)	1.9±0.5	(83)	4.5±3.0	4.4±6.1	(44)	4.9±0.6	2.4±2.9	(23)	10.0±8.9	1.9±1.9	(10)	4.3±2.6	0.5±0.6

The tables are very similar to Tables I.–IV. The three *O*'s stand in the same order of rank. Tu has decreased his range to 10: 11; D and Ta have hardly changed in this regard (11:15, 5:7). Ta has increased his average time of total sensation from nearly two to three seconds, and his record of recurrences is fuller, with longer times. On the whole we may say that Tu and D have behaved as they behaved in Series I., and that Ta, while he does not fall obviously out of line with his former record, may perhaps show the influence of some condition tending progressively to lengthen his periods of response.

Reports.—D's regular report is of a contact, or bright, clear pressure, more intensive than in Series I. This quality is sometimes obscured by, or passes into, a prick or wiry pain (13 cases) or a more diffuse tickle (8) or itch (15). Usually the intensity decreases steadily throughout the observation; sometimes it decreases and then increases (11); occasionally it decreases or remains fairly steady until the removal of the stimulus, when it suddenly increases (3). Of the 6 recurrences, one (of which *O* reports that it was probably imaginal) is of the original quality; 4 are diffusedly itchy; one has a tingling character.

Tu's regular report is of a light, delicate, ticklish contact. This quality is sometimes obscured by, or passes into, a bright pain (8). Tu now reports comparatively few visual images; we get, in exchange, reports of intensity. The contact usually disappears gradually; sometimes (9) its intensity decreases and then increases. All three of the recurrences are of the original quality, a ticklish contact.

Ta regularly reports a weak but clear (or very moderately intensive and clear) pressure, which either gradually fades out or (in 22 cases) passes, for the most part abruptly, into something bright and ticklish. Of the 75 first-recurrences, 57 are described as faint, vague, diffuse, unsteady, ticklish; 9 begin in this way, quickly become clearer and more intensive, and relapse into the first stage; 7 begin as dull, heavy and diffuse, and pass into the familiar vague tickle; and 2 only begin as 'concentrated.' Once a sensation of cold is remarked. All of the later recurrences are termed, comparatively, fainter, more obscure, more diffuse, fluctuating; all are of a ticklish quality.

We had hoped that, with change of the position of the arm from prone to supine, the two members of our 'irregular' group would give results comparable with those obtained from D, Ta and Tu. This hope was not fulfilled. In the first series taken under the new conditions with H and M, typical 'long' times (56 and 83 sec. respectively) occurred among the normal short times. Fortunately, one of M's reports in an early series gave us the cue to the situation: it appeared that M was making a voluntary effort to hold the

TABLE XII

OBSERVER H

Spot	Sensation	No.	Int.	A.-S. 1	No.	Int.	A.-S. 2	No.	Int.	A.-S. 3	No.	Int.	A.-S. 4
1	4.4±1.3	(9)	1.7±0.6	5.8±1.0	(4)	2.5±0.4	4.8± 0.7						
2	3.3±0.9	(9)	1.9±0.9	5.3±2.4	(3)	1.9±0.5	14.9±16.5	(1)	1.0	3.5			
3	3.3±1.3	(10)	1.4±0.4	7.4±3.0	(5)	2.9±0.9	6.1± 2.4	(1)	0.5	13.2			
4	4.0±1.4	(8)	2.1±0.8	5.4±1.7	(2)	2.7±0.0	4.0± 1.5						
5	4.5±1.5	(9)	2.2±0.9	7.5±1.7	(5)	4.0±1.6	4.8± 1.3						
6	3.9±0.9	(9)	1.8±0.7	4.9±1.9	(1)	3.8	3.1						
7	4.0±1.2	(8)	1.3±0.4	7.3±3.5	(4)	6.9±6.3	6.0± 2.0	(2)	2.7±2.7	14.9±9.6	(1)	6.5	11.4
8	3.4±1.5	(8)	1.6±0.4	6.2±1.3	(2)	3.6±1.0	4.3± 1.7						
9	4.1±1.2	(7)	3.1±2.0	12.2±8.2	(2)	1.3	5.1± 1.4						
10	3.9±1.4	(8)	1.6±5.8	7.3±2.3	(4)	3.1±0.6	5.0± 1.4						

TABLE XIII

OBSERVER M

Spot	Sensation	No.	Int.	A.-S. 1	No.	Int.	A.-S. 2	No.	Int.	A.-S. 3	No.	Int.	A.-S. 4
1	2.0±0.29	(8)	4.3±3.2	7.0± 9.1	(2)	13.3±1.6	0.7±0.5	(1)	16.0	0.9			
2	1.8±0.34	(8)	3.6±2.1	5.3± 6.4	(6)	3.6±1.3	2.1±2.4	(3)	9.3±7.4	1.8±0.9	(2)	7.3±4.1	0.2±0.03
3	1.7±0.38	(8)	2.9±1.8	1.4± 1.3	(3)	3.2±0.4	0.4±0.2	(3)	27.3±2.3	6.6±6.1	(1)	5.1	0.2
4	1.8±0.54	(8)	3.7±3.0	8.9±10.4	(4)	5.1±5.2	3.6±4.5	(2)	12.2±9.4	1.2±1.7	(2)	2.6±1.2	1.5±0.07
5	2.3±0.77	(9)	5.8±4.5	2.8± 2.5	(4)	3.6±1.1	1.6±0.7	(2)	1.8±3.6	0.7±0.6	(2)	3.8±1.2	0.7±0.05
6	2.4±1.00	(9)	6.4±4.7	2.5± 2.6	(5)	4.6±3.7	4.2±4.6	(2)	6.4±5.2	2.2±2.0			
7	2.1±0.65	(7)	3.4±2.0	1.5± 1.9	(4)	2.4±4.3	0.3±0.1	(2)	8.3±6.4	3.5±0.1	(2)	5.2±2.2	0.3± 0.10
8	1.8±0.36	(8)	4.2±1.2	6.6±10.1	(5)	6.9±2.3	2.6±1.8	(2)	2.8±0.8	1.3±1.2			
9	1.9±0.23	(9)	4.8±3.0	2.1± 2.2	(6)	3.8±1.1	0.9±1.0	(4)	5.2±1.2	1.7±1.5	(1)	5.4	0.5
10	2.0±0.40	(9)	4.2±3.7	13.2±16.0	(5)	4.5±1.4	4.4±5.1	(2)	11.1±7.1	7.3±7.1			

TABLE XIV

O	No.	Sensation	No.	Int.	A.-S. 1	No.	Int.	A.-S. 2	No.	Int.	A.-S. 3	No.	Int.	A.-S. 4
H...	(100)	3.9±1.3	(85)	1.8±7.6	6.8±3.2	(32)	3.4±1.5	6.0±2.7	(4)	1.7±1.3	16.6±8.9	(1)	6.5	11.4
M...	(100)	1.9±0.5	(83)	4.5±3.0	4.4±6.1	(44)	4.9±0.6	2.4±2.9	(23)	10.0±8.9	1.9±1.9	(10)	4.3±2.6	0.5±0.6

sensation and 'after-image' as long as possible, was seeking by 'effort of attention' to prolong her experiences. We accordingly modified the instruction given to H and M, to the effect that they were to take the stimulus passively, receptively; to be on the alert to signal what should 'come,' by way of sensation and after-experience, but not to try to 'hold' or to 'bring' these experiences. *From the time that this instruction was introduced, not a single 'long' time appeared in the records of the primary sensations.* Tables XII–XIV. show, in the usual form, the results of 100 observations.

M's times of primary sensation have fallen to an average of 1.9 sec., *i.e.*, to a value of the same order as the times of D in both sets of experiments and of Ta in Series I. H's average is higher, nearly 4 sec. Both *O*'s show many recurrences.

Before we discuss the effect of the new instruction, and the light which the consequent regularization of the times throws on the experiment as a whole, we present in summary the reports of H and M in this Series II.

Reports.—H's regular report is of a neutral pressure, of very limited area. The quality often passes into, or is obscured by, tickle (25); sometimes (10) a pain appears, 'quality baffling,' 'quality hard to tell.' Usually the intensity decreases steadily throughout the observation; in 14 cases H notes an increase followed by a decrease. There is in this Series no report of an increased intensity at the moment of removal of stimulus. Most of the 85 first-recurrences appear as a weak, lively, diffuse, indefinite contact, fluctuating in intensity; 15 are reported as actual tickle. There is one instance of 'pain.' The later recurrences are recorded as very weak, light contact; diffuse, fuzzy, fringy, indefinite in area. They seem to be weaker even than weak tickle (which is reported 4 times only), and to be little more than experiences of a 'something light and lively.' Occasionally *O* remarks: 'might have imagined it'; 'may have been illusion.' In three cases he says that the experience is not localized on the skin; it is 'perceptual.'

M regularly reports a light, bright, clear pressure or contact, of punctiform area; there are 19 cases in which a delicate prick, and 9 cases in which a tickle overlaps or replaces the contact. The quality either fades out gradually, or shows an increase of intensity followed by a steady decline. There is no terminal increase, though in a few cases the primary sensation drops out abruptly.

The reports of the recurrences are brief and very often doubtful. Three times only there is a blend of very weak pressure with tickle; once only a blend of very weak pressure with prick. Usually we have the adjectives fainter, weaker, indistinct, not localized, not cognitively clear, diffused, indefinite; not certain, not sure, doubtful; and such phrases as 'perhaps imagined,' 'not sure whether in sensation or imagination,' 'may have been pulse wave,' 'might not have been there.' Occasionally recourse is

had to visual imagery, with a perceptual consequence: 'visual image gives meaning of localization;' 'contact, not clear; visual image comes in with eye-movement, to localize'; 'weaker; gradually diffuses; visual image of *R* going down and moving off.'

If in the light of these results we now look back over the course of the entire experiment, we see (1) that D and Tu preserved stably the attitude which we had hoped to induce by instruction. (2) Ta, on the other hand, was impressed by the injunction to 'be ready for a recurrence'; his faith in *E* led him (as he told us later) to believe that if recurrences were mentioned, recurrences would come; he began to observe with an expectant set for recurring after-sensations. Hence in Table IV. we have a normal primary sensation, but an undue number of recurrences. Table XI. is then simply an exaggeration of Table IV.; it appears probable (though we cannot be certain of the point) that Ta's expectancy has extended to the primary sensation, and that the recorded average time (3 sec.) is somewhat too large. (3) H and M, finally, interpreted the instructions to mean that they were to take up an active attitude to their experiences, to secure and to maintain all the cutaneous sensations possible.[1] They give, accordingly, (*a*) highly irregular times for the primary sensation, times which jump from the normal to three or four times the normal's value, and thence to times which are extremely large. They report (*b*), even with their normal times, a high percentage of recurrences. They insist (*c*) on the difficulty of distinguishing between after-experience and imagination. And they have recourse (*d*) to perceptual construction, in order (by the aid of visual images) to anchor the fleeting and doubtful after-experiences. When the instructions are changed, they give (*e*) no more 'long' times for the primary sensation. M's average of 1.9 sec. suggests that her average in Table VII. (like Ta's in Table XI.) is affected by her general attitude. We may guess, though we cannot be sure, that both of H's averages, 2.88 and 3.96 sec., are for the same reason too long. The *O*'s (*f*) also show the continued influence of their original set in the high percentage of recurrences; but their reports have changed. H's recur-

[1] The same thing holds of P, so far as her results go.

rences, after the first, are too weak and faint even to be called ticklish; and M's recurrences are all so indefinite that she hardly ventures to give them a qualitative characterization.

We see, therefore, that the question of attitude or *Einstellung* is here of primary importance. Quantitatively we have a fairly satisfactory accordance of results with conditions. Qualitatively, to be sure, the problem is less simple. We know too little of cutaneous imagery to be able to say definitely wherein the recurrent experiences consist. Some of them, we may suppose, are true after-sensations of pressure. Others may rest upon some faint, ordinarily subliminal sensation, now raised to maximal clearness by *O*'s attentional attitude, and supplemented or embroidered by imagery. Others again may gather about an intercurrent sensation of tingling or warmth or cold. And yet others may be wholly imaginal, the result of autosuggestion. Our experiments do not permit us to decide, or to assign percentages; specially directed observations are required.[1]

We return to Dimmick's experiments. Dimmick's first observer C gives as average time of primary sensation 1.44 sec., interval 0.78, after-image 6.15 . We are not told the precise frequency of the after-images, but they seem to have been rather the rule than the exception. This difference between C and our own D and Tu may have been (like that of Ta) due to C's reading of his instructions; apart from it, there is close agreement.

Dimmick's second observer R gives an average primary sensation of 11.41 sec., interval 6.7, after-image 44.39. It will be recalled that the lowest of H's and M's 'long' times were about 10 sec.; there was a jump from the normal to this abnormal value. We may suppose, perhaps, that R observed more or less consistently (his *MV* was 10.45) at this first level of prolongation, and that he was prevented by some auto-suggestion or suggestion of instruction from proceeding to longer times. It is possible also that his after-image, recorded as a whole, was oftentimes a fluctuating and interrrupted experience.[2] These interpretations are, of course, guesswork, but they are guesswork based upon results gained with longer series and from a larger number of *O*'s.

We have little doubt that the tickle into which the primary sensation may merge is a phenomenon of adaptation; the initial contact brightens and lightens, and the

[1] A. S. Edwards ('An Experimental Study of Sensory Suggestion,' *Amer. Jour. Psych.*, XXVI., 1915, 124 f.) finds anticipatory images of touch which simulate sensation. J. R. Angell (*Psych. Mon.*, 53, 1910, 79, 94 f.) notes individual differences and proposes some simple tests. Hayes (*op. cit.*, 33 f.) makes brief preliminary reference to attention, but in her own work appears not to have regarded 'subjective' conditions.

[2] Our observer M reported only five continuous after-sensations of the 'long' type; they ranged from 40 to 170 sec.

quality becomes ticklish. We may thus account for the frequent appearance of tickle in the reports of Dimmick's short-time observer C. For the rest, there is always the question of practice. Tu's pricks, *e.g.*, may have been true pressure-spot responses; an *O* has to learn by experience the nuances of the pressure-sensation; but the pricks of the more practised *O*'s probably derived from concomitant stimulation of pain-spots.

Series III.: Forearm Supine, Blank Experiments

The care which our *O*'s took to report imaginal characters and resemblances puts it practically beyond doubt that the primary sensations were in fact sensational. We thought, nevertheless, that it would be worth while to add a few series which included blank experiments, in order to determine whether an anticipatory image might arise, clear and strong enough to create illusion. Since the introduction of a single blank in the series of 10 might possibly sharpen *O*'s attention, and thus lead to results not comparable with those of the preceding series, we introduced three or four blanks, placed at haphazard. Three to five series of the regular type (30 to 50 observations; all 10 pressure-spots) were secured from the 5 *O*'s.

D, H, M and Tu reported 'Nothing' every time that the *R* was omitted. Three of these *O*'s made no comment; the fourth remarked—what the others very probably thought—that something must have gone wrong with the apparatus. Ta went successfully through a first series. It happened that the first 'observation' of his second series was a blank; on this occasion Ta pressed his key about a second after the *R*, had there been one, would have been removed, and reported: "Nothing at first; then something very weak and faint." Some 3 sec, later he released the key, with the comment: "Don't know when it went away." Three further series passed without error. Ta's positive report (due, as it seems, to the suggestion of the first observation in a series; we may recall Ta's suggested rendering of the formula of instruction) is valuable; it shows how an after-experience may arise under the influence of expectant attention.[1]

[1] We may remind the reader that there were 8 cases, scattered through the total experiment after the introduction of the reading glass, in which spots failed to respond to stimulation. As it happened, the scattering included every one of the 5 *O*'s.

We conclude, then, that the primary sensations were, as we had supposed, genuinely sensational, and that their somewhat undue length in certain of our tables indicates simply their maximal prolongation by attention.

Conclusions

A. If *O* is in a passive, receptive attitude toward the experiment:

1. A stimulus-hair of 4.8 gr./mm. tension-value, applied to pressure-spots on dorsal or volar forearm, arouses a sensation of contact or light neutral pressure, which under adaptation brightens to a tickle. Sometimes (by stimulation of a near-by pain-spot ?) a wiry pricking pain is also aroused.

2. The normal duration of the primary sensation is from 0.75 to 2.00 sec. The sensation either decreases steadily in intensity throughout its course, or shows an initial increase followed by a decrease. Sometimes there is a sudden intensive increase at the moment of removal of the stimulus.

3. After-experiences are few and brief.

B. If *O* assumes an active attitude, making a voluntary effort to secure as much cutaneous experience and to maintain it as long as possible:

4. The normal primary sensation may last on the average 4 sec.

5. After-experiences are numerous and often of considerable duration.

6. Abnormal primary sensations, lasting as long as 200 sec., appear mixed irregularly with the normal.

7. There is a tendency to resort to perceptual construction.

C. The discrepant results obtained by previous experimenters under similar objective conditions are therefore, in all probability, due to variation in the attitude of their *O*'s.

D. While there is no doubt that true after-sensations ('after-images of pressure') occur, there seems also to be no doubt that free or tied pressure-imagery plays a part in the formation of cutaneous after-experiences. A systematic study of cutaneous imagery is needed.